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THE Psychological Review

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THE PSYCHOLOGICAL REVIEW

BEHAVIOR AS A CATEGORY OF PSYCHOLOGY[1]

BY JAMES R. ANGELL

University of Chicago

Two years ago at the meeting of this Association we attended the obsequies of the term 'soul' as a member of the vocabulary of psychology. At that time some of our philosophical colleagues put in a plea for leniency, but the psychologists for the most part were agreed that for their purposes the term had outlived its usefulness. It should be noted that this verdict was not so much legislative as historical. It did not attempt to forestall the actual march of events. It simply recorded the demise of the term as already *fait accompli*. Nor was the verdict concerned merely with a question of terminology. It marked the passing of the problem with which the term was concerned. Religious and metaphysical thought will no doubt continue their interest in the soul, but for psychology in its technical aspects, the chapter is apparently closed.

The term 'consciousness' appears to be the next victim marked for slaughter and as one of the claimants for its fading honors we meet the term 'behavior.'[2]

[1] A paper delivered before the American Psychological Association at its annual meeting held at Cleveland, Ohio, December 31, 1912.

[2] In a paper presented to the Minneapolis meeting of the American Psychological Association in 1910 I said: "But it is quite within the range of possibility, in my judgment, to see consciousness as a term fall into as marked disuse for everyday purposes in psychology as has the term soul. This will not mean the disappearance of the phenomena we call conscious, but simply the shift of psychological interest toward those phases of them for which some term like behavior affords a more useful clue.' Cf. the suggestive paper by Professor E. P. Frost 'Can Biology and Physiology Dispense with Consciousness?' PSY. REV., Vol. XIX., p. 246.

Consciousness has been attacked from two directions. Our philosophical friends have been for a long time pointing out the spurious character of its claim to an unique place in the universe and, inverting the Berkelian position, not a few of them have reduced consciousness to one among the many relations sustained to one another by physical objects. Others less drastic have contented themselves by exhibiting the ambiguities and anomalies contained in the conception. The difference between feeling and awareness, the distinction between consciousness and self-consciousness, the contrast of immediate with reflective consciousness, these and a dozen other differentiations have been exploited as the generators of confusion and error. As might be anticipated, the cures proposed for these ills are various, agreeing only in the one point that we are urged to adopt the usage proposed by the author whom we chance at the moment to be reading. Attacks of this character are essentially frontal in nature. There is in progress, however, a wide flanking movement of another sort.

The comparative psychologists have from the first been vexed by the difficulty of ascribing to animals conscious processes of any specific kind in connection with intelligent behavior. Animals give unequivocal evidence that they carry over the lessons of experience from one occasion to another. Upon this all are agreed. Whether they have conscious memory such as human beings possess and whether we are justified in assuming this to be true in the light of the evidence presented, is quite another matter and raises a question upon which experts differ. Obviously, for scientists engaged in this field of investigation, it would from many points of view be a material gain, in convenience at least, if the possible existence of consciousness might be forgotten and all animal behavior be described purely objectively. Nor has there been, so far as I am aware, any general objection to this proposal. To be sure, one still requires in describing this behavior, to distinguish between that which is hereditary, instinctive and reflex on the one hand, and that which represents individualistic 'acquired' adaptation to environment

on the other. Needless to remark, it is primarily with behavior of the latter kind that consciousness has been commonly assumed to play its chief part.

In studying animals it has naturally been easy to fall into modes of describing their activities which presuppose the presence of consciousness in forms resembling that of man under similar conditions. The more penetrating criticism of recent investigators has disclosed the subtle dangers involved in such an assumption and has shown how difficult it is to secure evidence unequivocal in its indications of processes identical with human mental activities. This circumstance has inevitably bred in many comparative psychologists that strong disposition already mentioned to shun the term consciousness on the ground that it is superfluous for an intelligent apprehension of animal behavior and that such terms as 'acquired' and 'instinctive' behavior may serve every purpose in reporting all of which we can really be confident, while avoiding any encroachment upon the dangerous regions of speculation.

It is furthermore not unnatural that finding it practicable and convenient, as undoubtedly it is, to waive reference to consciousness in matters relating to animal behavior, the tendency should manifest itself to pursue a similar line of procedure in dealing with human conduct. This tendency does not so much represent any formally recognized program like that of our world-reforming realists, as it does a general drift occasioned by several different causes and proceeding from several different sources. Its informal and unself-conscious character is probably indicative of a more substantial and enduring basis than belongs to movements more carefully and more purposely nurtured.

The revolt against the domineering claims of introspection as the alpha and omega of psychological method—a revolt which in America found one of its earliest and most pungent expressions in Professor Cattell's address at the St. Louis Congress—constitutes one division of the tendency I have in mind. We have been told by many of our recognized leaders, and with particular clearness by the honored president of

our association for 1912, that we can and do secure many of our most important forms of knowledge about the mind, not by introspection but through means as objective as those by which we measure time and space. Obviously, such knowledge must be gained by observation of the physical conduct of the individual or group of individuals whom we study. As such it involves the study of behavior as truly as does the study of animal actions, and it makes no more necessary use of the concept of consciousness.

Here belong such achievements as social and racial psychology can offer. Here, too, may be found much of sociology, economics, and history. The behavior of the mob, the execution of the ceremonies of religion, the various psychic and social circumstances surrounding the development of the family and clan, the peculiarities of sex and race, these are all matters of human behavior which lend themselves in large measure to objective description as well as to objective investigation, and the results so gained may not only claim a validity as great as any ordinary introspective observation, they are often in the very nature of the case results to which introspection in any usual sense of the term affords no adequate approach.

Again our so-called functional psychology, in one at least of its embodiments, has persistently emphasized the entire act from sense organ to muscle as the significant object of attack and has resolutely set its fact against any study of consciousness which divorces it from action and especially such study of it as consists in the search for structuralistic elements as the final *terminus ad quem* of psychological investigation. The exponents of this trend have refused to face the mind-body problem in the conventional dualistic manner, with its inevitable discontinuity of mind and body actions and have treated the distinction involved as one falling functionally inside the life cycle of the individual organism. Such a view obviously renders it easy to welcome a category like behavior which accents in its very first implication the fact of objective action. Allowing for some conservative reservations, its general tendency would be in

the direction of sympathetic acceptance of the behavior concept as a general term under which to subsume minor distinctions in modes of action whether conscious or unconscious.

The current doctrines of conscious attitude clearly lend themselves to incorporation in such a movement. The same thing may be said of the psychology of relation—a subject of so much interest in current writing. Even the imageless thought doctrine might conceivably find a home here. Certainly the very terms "attitude" and "relation" suggest modes of behavior, whether one conceives them in purely psychical form or in physiological ways.

It is no part of the purpose of the present paper to scrutinize the attack of the philosophers on consciousness. In due course whatever is empirically usable in the results of this assault will be taken up into the body of our scientific methodology. But the time is certainly ripe to invoice some of the more flagrant defects of 'consciousness' as a working concept of psychology and to this end we may profitably devote a few moments.

It is an interesting fact (if it is *really* a fact, as I believe it to be) that those psychologists who have kept closest to a detailed study of the actual operations of the mind and especially those who have worked most exclusively in the laboratory have been least disturbed by the frailties in the conception of consciousness. When you are experimentally determining the special form of imagery which your memory employs in a given situation, it is a matter of rather remote interest whether or not ambiguities lurk in the notion of consciousness. And the same thing is true when one is trying to ascertain the fidelity and the permanency of a memory process. One may even examine the mental complexion of an act of choice and find oneself quite unperturbed by ulterior considerations concerning the nature of consciousness. And so it is with the rank and file of strictly analytical problems. But the moment one begins to inquire how the mental act of choosing is converted into the neuro-muscular act of speaking one's mind, questions concerning the nature of consciousness come tumbling down like heaven's rain upon the just and the unjust.

Now occasional fortunate psychologists have found their somewhat simple interests entirely satisfied to play with the analysis of mental acts taken merely at their face value as mental, being content either to waive all question of their relation to bodily processes, or to accept parallelism, or perhaps to assume a naif interactionist attitude much after the manner of common sense. But the rank and file have been more sophisticated, and having eaten of the tree of knowledge, they have as usual suffered the pangs of spiritual indigestion. They have not contented themselves with a mere analysis of conscious states as such. They have inquired into both their antecedents and their consequences and in both directions they have harvested trouble and perplexity. Indeed, a few—and these for the most part are ravening philosophical wolves arrayed in the sheeps' clothing of psychologists—are inveterate investigators of the immediate implications of consciousness, self-consciousness, and the like. We may be exonerated of any intentional discourtesy if we observe that it requires no very high order of ingenuity to raise questions on this line whose successful answer implicates entire philosophical systems. In any case, the point to be made here is simply this, that one may carry out the program of a purely analytical psychology until the descent of the eternal twilight in which all cats are gray, without once feeling distressed by the improprieties of consciousness. But to overstep the limits of these confines in any direction and especially to attempt an understanding of the method by which one has come to his facts, and much more the way in which the mental and the physical come in touch with one another,—as obviously they do, both in sensation and volition,—this is to let loose a very Pandora's box of intellectual plagues whose ravages one is powerless to stay.

It is because the conception of consciousness has been at once too specific and too vague that it has led to such endless difficulties. It has been too vague in its failure to distinguish such differences as awareness and feeling, in countenancing and encouraging the antithesis between consciousness and self-consciousness, in permitting the same term to be

used to describe on the one hand the cessation of all mental activity, as in the phrase 'he lost consciousness,' meaning he passed into coma, and on the other, the mere obliviousness to a particular impression, as in the expression, 'I was not conscious of his gesture.' It has been too specific in that it has carried with it the notion of some kind of non-material entity, by some authorities alleged to be space-occupying, by others defended vigorously as non-spatial, by yet others regarded as receiving and giving off energy in exchange with physical sources of energy, by not a few regarded as wholly insulated from direct contact with the physical world and yet in some miraculous way reflecting the events in that world. Whether we shall gain or lose in fleeing from these ills to others that we wot not of, remains to be seen.

In considering the claims of behavior as a substitute for consciousness it will be well to safeguard ourselves against certain prepossessions gained from temporary conditions in our intellectual world. To most of us I am sure the term comes with a biological connotation, but we must not forget that chemistry discusses the behavior of the molecules, that astronomy relates the behavior of comets and that consciousness the despised may not only be regarded in its entirety as a mode of organic behavior, but that within its own jurisdiction it reveals innumerable variants each of which may be regarded as a special form of conscious behavior, *e. g.*, simple apprehension, judgment, desire, and aversion. In other words, if we are to accept the somewhat *naif* and uncritical biological use of the term, we must be prepared to expand or contract it as exigency may require.

Let us imagine our psychologist—or our behaviorist, as we may shortly be calling him—starting to define his field.[1]

[1] I regret that Professor Watson's brilliant article pleading for the discarding of all subjective method in psychology (PSY. REV., Vol. XX., p. 158) had not come to hand before this paper was written. I cannot now take up in detail any of its contentions, though I shall hope to do so at some later time. In general I should recognize cordially the service rendered by so courageous and lucid a statement of creed, although a part of the program seems to me rather Utopian and impracticable and other portions appear to disregard somewhat obvious distinctions and difficulties. Meantime, as I indicate in the present paper, I am heartily sympathetic to most of the author's constructive, positive program for emphasizing objective methods in psychology. This I made clear in my paper before the Association at Minneapolis in 1910.

He might adopt some of the familiar old phrases and dub it the description and explanation of behavior. But at once we need qualifiers. What kind of behavior; stellar, geological, chemical? Obviously not. Physiological, zoölogical, mental? Yes and no. It might prove unprofitable to invade very far the realm of physiology, which has to do primarily with those metabolisms and their supporting mechanisms by which the organism maintains itself. Clearly, however, the line of demarcation between this territory and that preempted by general zoölogy in its divisions devoted to the study of animal behavior, is anything but sharp. The mechanisms of the optical reflexes are matters primarily of physiology. But their actual employment, their utility, their origin, etc., are matters belonging to zoölogy. In zoölogy we find recognition at once of innate and acquired forms of behavior. Here then, as we have before noted, would open up our new field. Mental life, conscious process, as our psychologists have dealt with it, has had to do with reactions which were mainly concerned with new individualistic adaptations. The behavior which we should study in man would be in part therefore, the old instinctive behavior, but in part this new personalisticly adaptive behavior. As a program this is entirely intelligible. Indeed, it sounds very much like our current psychological texts—in its purposes anyhow, if not in its terminology.

But now the crucial question arises: Can we carry out this program? Can we give an account of human behavior leaving out all purely subjective or conscious reference and above all—as the most crucial test of some of the newer ambitions—can we dispense wholly with introspection in gaining such objective descriptions?[1] Let us examine some typical situations and see what occurs.

[1] Cf. Professor Robert MacDougall's interesting paper in this journal (Vol. XIX., pp. 386–403, especially pp. 396–7 and 400) in which he defends the position that psychology deals with facts that '*cannot* be objectively discerned' although 'their existence may under certain conditions be inferred.' It must be apparent that when the investigation of mental life becomes purely objective, the term 'psychology' will have lost most of its propriety as a designation for such a science. Indeed, the exclusion of the psyche from 'psychological' consideration has proceeded so far that the fitness of the term for its current use may even now be challenged.

Analytical and experimental psychology has made much of the resolution of color experiences into simple qualities. Can such distinctions be stated in terms of overt motor behavior and can they be attained without the aid of introspection? Obviously one may give a qualified affirmation to the first half of the question. The differences in color qualities may be indicated by differences in motor behavior, and when remarked, do commonly secure such recognition. Not only does one give a different name to varying hues, one also reacts in a dozen different ways to a dozen different colors. Certainly it would be possible by a mere study of a man's behavior to ascertain what his capacities are for color discriminations. To determine this with nicety would require patience and experimental skill, but it could certainly be done. Indeed, it *is* done in every psychological laboratory, and with animals as well as with men. But we must not forget that whenever we avail ourselves of language as a mode of approach to objective behavior, we are apt to find ourselves compromising with introspection, for such language may simply report mental states in the ordinary introspective fashion.

In the same way, one may determine by objective methods the limits and peculiarities of perceptual capacity, the tenacity and scope of memory, the range and subtlety of imagination, the degree of available concentration, the power and flexibility of reasoning, the firmness of purpose, the resources of character and so forth and so on. If one inquires how this may be done, the reply is 'through the refinement and elaboration of just those methods by which men have always tested one another, *i. e.*, their practical achievements in the face of practical exigencies.' This man's memory is better than that man's and the fact of his superiority is common knowledge. This man's imagination is more active than his neighbor's, as is witnessed by his readier wit, inventiveness, artistic productivity or what not. This man's courage and resolution surpass his friend's, as is clear from the slightest survey of their respective careers. All these differentiations lend themselves to objective test. To be sure some of these tests are

extremely crude. But they might be improved and in any case the point is that the important distinctions in mental capacity and function may be observed without direct resort to introspection; in other words, they may be judged in accordance with objective behavior and accordingly the conscious processes implicated in them may be disregarded as objects of study in favor of this behavior.

A closer observation of certain situations shows that however valid this conclusion may be in a general way, it encounters serious practical difficulties at not a few points. For example, here are two persons memorizing a given material. They do equally well. Hence we may judge what? That they have the same kind of memory? Not at all. Simply that tested by this task they are equally effective. Introspection may reveal the fact that the two persons work by wholly different methods. Then it should follow either that the differences are not significant for behavior and may accordingly be disregarded on pragmatic principles, or else that we must find other tests which will disclose in unequivocal objective form the existence of the differences at issue. In theory this reply is adequate, but in practice, as every one knows who has made experiments upon memory or upon imagery, it is at present rather futile. Thus far no such tests have been devised and there is at present no great prospect of such. The mind is provided with so many and so flexible shunt systems, it translates from one material into another so quickly, that it promises, for a long time to come, to baffle the skill of the experimental 'behaviorist' at many points. It remains for him then to urge the trivial and insignificant character of differences so elusive, and perhaps he is right. But right or wrong, there are at present innumerable instances of which this is one—processes of rational reflection afford others—where objective methods are at present only competent to give rough surface facts.

What then is to be said of the second half of the question, that which relates to the gaining of evidence? Are we within measurable distance of being able to discard introspection, even supposing it were thought desirable to do so? We are

all familiar with the hard things which have been said about introspection. We know the old charge that psychology could never be a science because it must forsooth depend upon introspection—a process which is not only susceptible to all the ordinary errors of observation, but which, in the indictment, suffers from the further and fatal defect that in its very nature it results in a destruction or distortion of the mental fact which the observer intended to study. And we know the ingenious attempts to turn the edge of this criticism by exploiting the method as one of retrospection or one of construction. We have faced, too, the more *fin de siècle* and more empirical criticism of the psycho-analytic school with its insistence upon the general unreliability of introspection as evinced in psycho-analytic procedure. Meantime, if one offers up as a burnt sacrifice, the word 'introspection,' everybody, plain folks, psychologists, and psycho-analysts, alike, believe themselves to be in immediate possession somehow or other of reliable information concerning their own feelings, sensations, and intentions. In all this conviction they may of course be deluded. But at least they cherish the conviction and not primarily on any grounds of objective behavior, but rather on the ground of their direct conscious apprehension—call it what you will. Direct access to these facts of feeling is gotten only in this way. Indirectly we may get at them in many ways, as we do in studying history or in watching the actions of others. Our question must then reduce itself to another, *i. e.*, is it scientifically worth while to get at these purely subjective facts? Can we not get all that is of permanent significance and worth by other means? My own pain is a matter of compelling interest to me personally, but to science it is simply one among many pains and of interest only as it can be given context with its generic antecedents and consequences. The one type of interest is ephemeral, the other is relatively permanent.

If we set out resolutely to learn all we can of human behavior without recourse to introspective methods, we find a situation of substantially the following type: we know that stimulations of various physical kinds fall upon the sense

organs. We know that some of these issue at once in muscular or glandular activities. We know in some cases the intermediate neural pathways involved. We know something of the changes occasioned by muscular activities in the physical world. We know that some muscular activities of a coördinated and efficient sort arise when no apparently adequate sensorial stimulus is at hand. We know that present behavior displays modifications which can only be attributed to previous behavior. We therefore know that in some way the organism stores up its experiences and creates the possibility of a variability in reactions such as baffles all prediction. But what happens between the time a stimulus affects a peripheral organ and the later time at which some reaction is made, we can often only judge with approximate accuracy provided the individual concerned tells us what has passed in his mind during the interim. The same thing is true of those reactions which are made in seeming independence of any immediate sensorial excitation. In other words, we have not at present any technique for ascertaining the train of neural units intermediate between a specific sensorial stimulation and a specific delayed response. This gap we must bridge with information gleaned from essentially introspective sources or else leave it open. As a matter of fact, however, one could not long remain in ignorance of the *general character* of these intermediate happenings unless one were prepared to forego all conversation with other human beings and to eschew all contact with poetry and fiction, for from all these sources a steady rain of introspective description—good, bad and indifferent, false and true—is constantly falling: to say nothing of the poisonous introspective miasma always rising in one's own mind, however resolutely objective one may be.

All things considered then, we may reasonably question whether we can at present throw over introspection altogether, unless we are prepared to content ourselves with the two end terms of a series of events of which the intercalary links are frequently most complex and significant. These links are now often accessible to us only in mental terms.

The type of criticism indulged by our psycho-analytic friends ought not to be allowed to confuse the issue. To say as they do that a reagent's introspective *explanation* of a specific reaction is generally worthless might be accepted as true for the sake of argument and still leave entirely untouched the fact that his introspection is entirely valid for certain other purposes, such, for example, as declaring whether or not the stimulus word stirred up a visual image in his mind. Introspection under its most pretentious forms has its limitations like every other method. Those limitations cannot in fairness be generalized into a universal defect. The explanation of a reaction is one thing. For this introspection may or may not afford an accurate basis. To state what occurred in one's inner experience at a given moment is quite another thing and for this purpose trained and controlled introspection is often entirely adequate.

What becomes, on such a program, of psychology as the science of self? What becomes of the entire system of moral and spiritual values and experiences? Two replies seem obviously possible. One is that these values will all be ultimately conserved and recognized, but that in place of descriptions couched in subjective terms, they will now appear described in terms of their objective equivalents. One suspects that the thrills of young love when so portrayed will present a somewhat clinical not to say mortuary appearance. But it always takes time to learn to speak a new language and if such acquirement spares us persistent annoyance, it may be worth while. The other reply is that these interests are to be turned over entirely to ethics, metaphysics and religion, which long ago preëmpted the larger part of them. We shall then be changing names rather than facts. Certainly we must not deceive ourselves into supposing that any such procedure as that we have been considering will lay the ghosts. Persons will still be interested in these problems of mind and morals and will seek their solution, under whatever flag they may march in so doing.

After all is said and done, something corresponding to consciousness in its vague common meaning does exist and

it is within its compass that the problems of science arise. We must be cautious therefore that in seeking for bettered means of knowing human nature in its entirety we do not in effect commit the crowning absurdity of seeming to deny any practical significance to that which is its chief distinction —the presence of something corresponding to the term mind— the one thing of which the fool may be as sure as the wise man.

We may agree then that in theory all and in practice much of our mental life might be stated in terms of objective behavior. To do this would involve trespassing rather freely on the preserves of biology, physiology, and neurology on the one side and upon those of the social sciences on the other. But such trespass is perfectly legitimate provided the trespasser is willing to face the chance that he may find himself annexed, appropriated, and in general swallowed up by the owner of the territory which he invades. When she abandons the stronghold of consciousness as her peculiar institution, psychology is moderately certain to find that as an autonomous government she has ceased to exist and has become a mere dependency of biology or some other overlord. But this change of allegiance is not likely to terrify many of her subjects. The point of view from which not a few of our recent psychologists have conducted their work would make such a shift of emphasis entirely easy. (I count myself among this number.) Whether it is worth while is another matter. Personally, I am disposed to think it is, for the reason that every step forward in that direction renders more obvious the actual vital significance of mental process and this happens to be the thing that interests me. I want to see just how ideas and feelings embody themselves in action and a psychology which makes objective description its main concern must inevitably further this interest.

When it comes to discarding introspection I demur.[1]

[1] I do not in this paper undertake to embark upon the troubled waters of definition. Suffice it to say that however introspection be defined and whatever merits and defects may be alleged to attach to it as a method for ascertaining facts, all, so far as I know, are agreed that we are directly cognizant of our own experience in a manner different from our indirect apprehension of the experience of others. Whatever this *direct* mode of approach may involve under final analysis, it may serve for the moment to

Introspection ought to be checked by every possible objective device, but even for one frankly and exclusively interested in objective behavior as such, it seems to me at present to afford information not to be gained elsewhere. Indeed, as was intimated above, we are saturated from childhood on with prepossessions, convictions, ideas, of which we cannot wholly divest ourselves if we would, which have introspection as their foundation and which enter in to color our entire conception of behavior.

At this point too we come upon one of those fundamental lines of cleavage in human interests which cannot be justified, but which nevertheless determine conduct and career. The person to whom mental process as mental process is the only fascinating and ultimately worthy subject of study is not likely to rest content with any such program as that depicted. He justly urges that to recognize and describe the *external expressions* of love, hate, and anger is as different from the actual experience of these thrilling emotions and from the description of them as immediately felt, as is the inspection of a good meal from the consumption of the same. To such an one any abandonment of introspection must seem a pitiful and mean desertion of the real object of worth. Whether this view permanently prevails or becomes an esotoric scientific cult, it is a safe prediction that we shall always have it with us.

It is a wise instinct which science has always followed to glean information wherever it can be found. Until it can be shown, as it has not yet been shown, that introspection is either fundamentally incompetent, or clumsily and viciously misleading, it is the part of good judgment to use it. Refine it, check it, train it, but do not throw away a good tool until

represent the sort of thing I have in mind by introspection. Cf. Professor Titchener's interesting articles in the *American Journal of Psychology* (Vol. XXIII., pp. 427, 485) and Professor Dunlap's indictment of current implications of the term in this journal (Vol. XIX., p. 404). I find myself largely sympathetic to the position taken by Professor Dodge in his paper on the 'Theory and Limitations of Introspection' (*Amer. Jour. Psy.*, Vol. XXIII., p. 214). Since this paper was written, Professor Bode's paper on Introspection has come to hand (*Jour. Phil., Psy. and Sci. Methods*, Vol. X., p. 85) in which he presents an interesting revision of the usual implications of the term and the process to which it applies.

you certainly have a better in hand. And do not forget that in much which offers itself as objective method, introspection is really involved either directly or indirectly.

Let us then bid the movement toward objective methods and objective description in psychology God-speed, but let us also counsel it to forego the excesses of youth.

JUDGMENTS OF SIMILARITY AND DIFFERENCE

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When an observer is presented with two stimuli and instructed to compare them with respect to some general property such as weight, size, pitch, affective quality, intensity, etc., it is apparent that he has fairly decided preferences or inclinations with respect to the form in which his judgment is expressed. Thus comparisons of weight may proceed in terms of either heaviness or lightness, comparisons of pitch in terms of either highness or lowness, comparisons of affective quality in terms of either agreeableness or disagreeableness. But experiments show that judgments in terms of lightness, lowness, shortness, smallness, faintness, etc., are very infrequent so long as the observer is left to his own inclination. These categories, which may be designated as "negative," since they imply the absence or a smaller amount or degree of some positive factor in the stimulus or situation, seem to be, if not more artificial, at least more unaccustomed than the contrasting and grammatically opposite 'positive' categories.

Conceivably these natural tendencies, inclinations, or judgment habits might exert an appreciable influence on the apperception of the two stimuli, and hence on the outcome of the judgment in cases in which the differences, though objective, were small. This point has not remained untouched in the technique of the psychophysicists. In an important chapter of his recent monograph Warner Brown emphasizes the fact that, in the comparison of lifted weights the judgment of difference depends upon the form of expression. It will be recalled that Müller and Schumann, and Müller and Martin made certain recommendations as to procedure in psychophysical experiments, as a result of related observations. Brown writes: "The results leave no

doubt that a difference in the framing of two propositions which are precisely equivalent logically will be a governing factor in making a comparison. Evidently no comparison is complete with the mere apprehension of the presented stimuli. These are apprehended in the light of other stimuli which have gone before, but even then the analysis is not complete without taking into account of what the observer has to do in the matter. Even the slightest differences in the task which he has to perform seem to govern to some extent his decisions."[1]

When Brown's report appeared the writer was in the midst of an investigation of judgments of 'subjective' type, such as are involved in the comparison, estimation and measurement of complex material such as handwriting, comic situations, arguments, appeals to instincts and interests, photographs, etc. One of the problems outlined in that investigation is that of investigating the influence of the category or form of expression on the outcome of judgments of similarity and difference, and of other pairs of logical or grammatical opposites, of analyzing the psychological relation between the two types of judgment, and of discovering the relative ease, consistency, and certainty of the various categories when the judgments are directed toward the same material, both in the case of the same observer and with groups of observers. The present study concerns itself with the first-mentioned pair of categories—*similarity* and *difference*.[2] This problem, in its own field, is closely related to that examined by Brown in the case of lifted weights.

The problem, in the writer's mind, grows at once out of the contradictory character of the few references available in the literature of judgment. The following references to experimental and general studies will illustrate the point, and raise more or less definitely the question at issue.

[1] See 'The Judgment of Difference,' Chapter VII., University of California Publications in Psychology, Vol. 1, No. 1, pp. 1–71, September 24, 1910.

[2] See also 'The Influence of Form and Category on the Outcome of Judgment,' *Jour. Phil., Psych., and Sci. Meth.*, September 12, 1912.

June E. Downey, 'Preliminary Study of Family Resemblance in Handwriting,' Bulletin No. 1, Dept. of Psychology, Univ. of Wyoming:

"In general a judgment of unlikeness is made with greater ease than one of likeness" (p. 49). "Toward the close of a series the judgments became judgments of dissimilarity. The records show that such a judgment is frequently made more easily than is a judgment of likeness. . . . There were subjects . . . who were more constant in their judgments of dissimilarity than in those of similarity, and who varied less from the average in the case of the latter. Some subjects . . . *first* selected the specimens most unlike the standard and then proceeded to find the similar hands by elimination of the unlike" (p. 20). "The judgment of unlikeness is on the whole an easier one to make than the judgment of likeness. There is considerable agreement among subjects as to the handwriting most unlike a given specimen" (p. 24), etc.

These statements are based on the variabilities of five successive trials by the same individuals, the instructions being "to arrange the writing specimens in the order of their likeness to a given standard" (p. 15). But if one is judging in terms of *likeness* one can not fairly speak of judgments of *unlikeness* resulting from such an experiment. It is assumed here that the category in which the judgment is expressed has no influence on the outcome of that judgment. But I shall show later that a judgment of unlikeness is not merely the reverse of a judgment of likeness, but a new kind of judgment. The 'least similar' is not therefore the 'most unlike.'

George V. N. Dearborn, 'Notes on the Discernment of Likeness and Unlikeness,' *Journal of Philosophy*, etc., Feb. 3, 1910, p. 57:

Reports a research which "aimed to help the analysis of the mental process by which we become aware of similarity and dissimilarity . . . judgments as to the likeness and unlikeness experienced in the case of a series of visual forms. . . . The method of experimentation in detail was simply as follows: The hundred blot cards (bearing blots of ink) being placed in order ten-square on the table before the seated subjects and the norm in its frame conveniently before his eyes and above the blots, he proceeded to select within fifteen minutes the ten blot-cards out of the hundred most similar in form or shape to the norm, and to place them one side arranged carefully and deliberately in the order of their judged similarity to the norm. Meanwhile the subject reported how he apperceived the norm and what he considered its most essential form-characteristics and peculiarities. These subjective notes were recorded and the numbers of the ten blots judged most like the norm, and in their chosen order. The time required for a selection satisfactory to the subject was also recorded, and at the end of the selection the reason why each of the ten had been preferred, concisely as possible. The process in the case of judgment as to unlikeness was precisely the same,

with the appropriate change in intention to keep dissimilarity instead of similarity in mind" (pp. 57–58).

Dearborn continues, "Ideal criteria (as distinguished from affective) gave more accurate results in the dissimilarity choices than in the similarity choices. This is as we should expect on logical principles. The awareness of unlikeness is an easier if not a simpler process apparently than that of likeness, for the change of consciousness is greater and so easier to appreciate. At any rate the sets of blots chosen as unlike the norm were much more certainly unlike it than were the 'similar' blots chosen like it" (p. 61).

There are two things to be pointed out in this connection. The first is the fact that in Dearborn's experiment the judgments of likeness and of unlikeness were directed toward totally or partially different stimuli, and hence the ease of the judgment as mere judgment is in no way indicated by his results. It may well have been that the dissimilar blots differed from the standard in more points than that number in which the similar blots resembled the same standard. In the absence of quantitative measurements of amounts of likeness or unlikeness, the relative ease of the two types of judgment can be made out only when the same material is employed in the two cases. The second point is that the assumption that the awareness of unlikeness is a simpler and easier process than the awareness of likeness seems to the writer to be completely gratuitous, until the difference has been experimentally demonstrated. The results of the present experiments indicate that the contrary is the case.

As opposed to the point of view suggested in the two articles just referred to, we find in other places frequent assertion of the more fundamental character of the judgment of resemblance, and the derived character and secondary importance of the judgment of difference. Thus Miss Macdonald, in her review of Preyer's 'Infant Mind,' says that likeness is more easily discerned than difference.

Titchener, 'A Text Book of Psychology,' p. 26, says: "We notice these differences (in human bodies) because we are obliged, in every day life, to distinguish the persons with whom we come in contact. But the resemblances are more fundamental than the differences. If we have recourse to exact measurements we find that there is in every case a certain standard or type to which the individual more or less closely conforms and about which all the individuals are more or less closely grouped. And even without measurement we have evidence to the same effect; strangers see family likenesses which the members of the family cannot themselves detect, and the units

in a crowd of aliens, Chinese or negroes look bewilderingly alike." That there may be a difference in the psychological character of the two judgments is suggested by the same writer's statement that "Reports of equality or identity are less frequently based on imageless comparison than reports of difference" (p. 534).

Bacon remarks on 'the peculiar and perpetual error of the human understanding to be more moved by affirmatives than negatives' ('Nov. Org.,' Bk. 1, Aph. XLVI.), May we not perhaps substitute in this statement the word 'similarities' for 'affirmatives' and 'differences' for 'negatives'?

Jevons, 'Principles of Science,' pp. 43 and 44, however, seems to mean that similarity and difference are only two forms of expression of one and the same judgment. "In every act of intelligence we are engaged with a certain identity or difference between things or sensations compared together." "We cannot, in fact, assert the existence of a difference without at the same time implying the existence of an agreement. "Agreement and difference are ever the two sides of the same act of intellect. and it becomes equally possible to express the same judgment in the one or the other aspect." "It is a matter of indifference in a logical point of view, whether a positive or a negative term be used to denote a given quality and the class of things possessing it." "But there are very strong reasons why we should employ all propositions in their affirmative form." "All inference proceeds by the substitution of equivalents and a proposition expressed in the form of an identity is ready to yield all its consequences in the most direct manner. . . . Difference is incapable of becoming the ground of inference; it is only the implied agreement with other differing objects which admits of deductive reasoning, and it will always be found more advantageous to employ propositions in the form which exhibits clearly the implied agreements."

Bergson, 'Creative Evolution,' p. 214, remarks: "Independently of all consciousness the living body itself is so constructed that it can extract from the successive situations in which it finds itself the similarities which interest it, and so respond to the stimuli by appropriate reactions." Also (pp. 44–46): "We must have managed to extract resemblances from nature which enable us to anticipate the future."

The last three references seem to agree on the proposition that psychologically, in real life, it is similarity that most interests us. If we perceive difference it is only for the sake of a search for similarity-conformity to type, interest, image, desire, etc. In handling coins the differences usually lapse in favor of the similarities, except in the case of the expert. To perceive differences requires special, sometimes professional training, and this is not necessarily because the differences are *smaller* than the agreements. They may be just as obvious, once they become interesting. We are seeking for agreements. In hunting the resemblance of the stubble to the form of a rabbit is more striking than its many points of difference. So in diagnosing disease we are strongly interested in certain diagnostic or prognostic features and accustomed to look for them, since *they* are significant in the

midst of infinite diversity of other factors. Just as we are prone to 'see only those instances which are favorable to the theory or belief which we already possess' (Creighton, 'Logic,' p. 250), so we tend to warp every perception toward the idea or image which we happen to have at the time. And just as in observing a race of men, the members of a profession, or a species of animal or plant life, we tend always to form a conception of a type or mode from which the separate members of the group shall vary the least, so in so artificial a task as the process of judging the separate magnitudes of an experimental series we tend to conceive a central value from which the total deviations of the different magnitudes shall be the least. The clearly demonstrated 'central tendency of judgment,' the so-called 'indifference point phenomenon' may be due largely to the fact that resemblances are more striking than differences, and hence all magnitudes approximating the type are assimilated towards it.[1]

The Present Experiment

The purpose of the experiment was to investigate the influence of the category or form of expression on the outcome of judgments of similarity and difference, to analyze the psychological relation between the two types of judgments, and to discover the relative ease, consistency and certainty of the two judgments when directed toward the same material, both in the case of the same observer and with groups of observers.

The material to be judged consisted of 35 specimens of hand-writing, each specimen written by a different individual, the individuals chosen at random. Each individual wrote, on a standard sized card, the words,

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One individual wrote two copies, one of which served as the standard by which the other 35 specimens were judged.

[1] See Hollingworth, 'The Central Tendency of Judgment,' *Jour. of Phil., Psych., and Sci. Meth.*, Aug. 18, 1910. Also 'The Inaccuracy of Movement,' Chapter III., on 'The Indifference Point.'

The same cards and the same standard were used throughout the experiment, which covered a period of 14 months.

The chief observers, 9 in number, were divided into three groups, designated by the words 'similarity 1st,' 'difference 1st,' and 'mixed.' Each member of the first group proceeded as follows: He was given the pack of 35 specimens, accompanied by the standard card. He was asked to arrange the cards in an order of *resemblance to the standard*, placing the most similar specimen at the top, the *next most similar* in the second place, and the *least similar* at the bottom, with the remaining cards in their appropriate intermediate positions. After completing his arrangement, for which he was allowed all the time desired, he was handed a sheet of paper and requested to give an introspective account of the criteria used in passing his judgments. A week later he was again given the cards and asked to again arrange them in an order of *similarity* to the standard. After this second arrangement he was given his introspection sheet and asked to note down any modifications of criteria observed in this second trial.

After another week the same observer was given the cards and asked to arrange the specimens of handwriting in an order of *difference from* or *unlikeness to* the standard, putting at the top of his list the card *most different*, at the bottom the card *least different*, etc. A fresh introspection sheet was prepared after this arrangement, and criteria noted without reference to the previous records. After a third week a second arrangement on the basis of *unlikeness to the standard* was made, and further notes made on the introspection sheet.

The 'difference 1st' group performed the experiment in the same way, except that their first two arrangements were in terms of *difference* and the last two in terms of *similarity*. In the case of the 'mixed' group an arrangement on the basis of similarity was followed by an arrangement for difference, or vice versa, before the second trial for the same category of judgment.

Only one of the observers (H. L. H.) knew the purpose of the experiment at the beginning. One observer (Str.) sus-

pected the purpose before his arrangements had all been made. Observer H. L. H. repeated the four arrangements 14 months after the first trials had been made. The intervals of one week seemed to be sufficiently long to eliminate any very decided memory effect except in the cases of the one card written in the same hand as the standard, and one other card which was strikingly different from that standard in almost every respect.

The place of each card in the various orders of merit was recorded for each observer. The data secured from such a procedure can be examined from many points of view. In the case of each observer the two orders for *similarity* can be correlated, and the consistency of such a judgment indicated by the coefficient of correlation. The same thing may be done with the two orders for *difference*. The orders for *difference* may be inverted and the reciprocal order thus obtained correlated with the original orders for *similarity*. In the same ways may be treated the final orders for both *similarity* and *difference* secured by averaging the arrangements of the nine observers. The three groups of observers may be compared with each other in all these respects. In the case of the final orders for both categories, the variability of the individual judgments may be computed for each card, and the categories and groups of observers again compared with respect to this *variability of judgment*. The arrangements of the various observers may be compared with the final orders secured from the group averages, and in this way the agreement of each individual with the group average (general judicial capacity) determined. Comparing these measurements with the correlation between the various trials of the same observer affords a measure of the relation between *personal consistency* and *general judicial capacity*. Various other interesting and perhaps exceedingly significant comparisons may be made, some of which will be later pointed out. All of these points of view will throw light on the psychological relation between the two categories of judgment, which it is the main purpose of the investigation to study.

The results of many of these comparisons and correlations

are given in the following tables. In computing coefficients of correlation the formula[1]

$$r = 1 - \frac{6(\Sigma d^2)}{n(n^2 - 1)}$$

has been used.

Table I. gives the coefficients of correlation between the various arrangement of each of the nine observers, along with the average coefficients for the group. In this table *S*1 indicates the first trial for *similarity* and *S*2 the second trial. *D*1 and *D*2 indicate the two trials for *difference*. Whenever *similarity* is correlated with *difference* the reciprocal of the *difference* order (the inverted order) is used.

TABLE I

CORRELATIONS BETWEEN THE VARIOUS ARRANGEMENTS BY THE SAME INDIVIDUALS

S, Similarity. *D*, Difference. The orders for difference were inverted whenever Similarity was correlated with Difference. The figures represent positive coefficients of correlation, by formula given in text.

Subject	*S*1 with *S*2	*D*1 with *D*2	Average	*S*1 with *D*1	*S*2 with *D*2	Average
L. S. H., *S*1st	.833	.813	.823	.639	.723	.681
DeN. "	.781	.572	.677	.619	.655	.637
Str. "	.700	.811	.756	.506	.767	.636
Rich., *D*1st	.856	.676	.766	.654	.740	.697
Bar. "	.748	.586	.667	.613	.653	.633
G. E. H. "	.916	.727	.822	.630	.754	.692
Hart, mixed	.756	.775	.765	.572	.784	.678
Kup. "	.771	.894	.832	.760	.911	.835
H. L. H. "	.744	.677	.710	.439	.495	.467
Average	.789	.726	.757	.604	.720	.662
Mean variation	.052	.087	.055	.065	.079	.061

Several points are at once disclosed by Table I.:

1. The correlations of the two arrangements according to *similarity* (*S*1 with *S*2) are greater than the correlations of the two arrangements for *difference* (*D*1 with *D*2). With six of the nine observers this is clearly the case. With three it is

[1] The formula, and, in part its derivation, may be found in Meyer's, 'Text Book of Experimental Psychology,' Chapter X., and also in Whipple, 'Manual of Mental and Physical Tests,' Chapter III. The introspections of the observers, in so far as they bear on the point of the experiment, are also given.

not true. Two of these three are in the mixed group, and in one of these cases there is no real difference between the two coefficients. The third exception to the rule is in the case of observer Str. who suspected the purpose of the experiment and whose introspective account states that he was disturbed by having read up on the subject. However, observer H. L. H. was aware of the purpose of the experiment from the beginning, he being in fact the writer, and his coefficients show the normal relation. Apparently the mixed order of arrangements introduces factors or tendencies not present with the other two groups. (See also introspections of observer Kup. under 'Difference.') Whether *similarity* or *difference* is judged first, five of the six observers in these two groups show considerably higher *personal consistency* when judging *similarity*. Averaging the nine observers yields a coefficient of .789 for similarity as against .726 for difference.

2. If there is no psychological difference between judgments of similarity and judgments of difference, if, as Jevons states, 'Agreement and difference are ever the two sides of the same act of intellect, and it becomes equally possible to express the same judgment in the one or the other aspect,' the inverted order for *difference* should show the same correlation with a direct order for *similarity* as do two arrangements for *similarity* or two arrangements for *difference*. The fact that the coefficients for similarity are higher than those for difference suggests that the two categories of judgment are not psychologically the same. But the case is still more apparent when these reciprical correlations are compared with the direct ones. Observe the correlations of $S1$ with the inversion of $D1$. With every observer these coefficients are smaller than those for two arrangements for similarity ($S1$ and $S2$). The average coefficient is almost 20 per cent. lower. And with seven of the nine observers these coefficients are also lower than the coefficients for two arrangements for *difference*, the average coefficient being 12 per cent. lower.

3. With every observer the coefficient for $S2$ with $D2$ is higher than that for $S1$ with $D1$, the average difference being 12 per cent. That is to say, with practice and repeti-

tion the two judgments come to resemble each other, and the inverted order for *difference* to agree more closely with the direct order for *similarity*. This, we may assume, accounts for the uncertainty shown by the members of the mixed group, with whom the two categories clashed more quickly than with the other observers, who had made two arrangements under one category before the other category was suggested. But even in these correlations of $S2$ with $D2$, six observers show less agreement than with the two arrangements for *similarity*. The average is some 7 per cent. lower than the average for $S1$ and $S2$, and about the same as the average for the two orders for *difference*. Averaging the direct correlations and comparing this coefficient with the average for the inverted correlations shows a superiority of 13 per cent. in favor of the former, and among the nine observers the only exception to this rule is Kup. in the mixed group, whose two averages are identical.

It seems to be clear then, that the two categories are not merely 'the two sides of the same act of intellect'; that different psychological processes are involved, processes so different that they modify the outcome of the judgment; and further, that judgments of *similarity* are made, if not more easily, at least with higher consistency than are judgments of *difference*.

Table II. gives the variability of the group averages for

TABLE II

THE VARIABILITY OF THE GROUP AVERAGES FOR THE VARIOUS ARRANGEMENTS
The figures are the average M.V.'s of successive groups of five cards.

Positions	Similarity 1st Trial	Similarity 2d Trial	Difference 1st Trial	Difference 2d Trial
1 to 5, inc.	5.18	4.46	4.70	5.40
6 to 10, inc.	5.44	6.78	6.42	7.90
11 to 15, inc.	6.76	6.88	7.76	7.77
16 to 20, inc.	6.34	7.72	8.16	7.58
21 to 25, inc.	6.34	6.96	6.78	5.82
26 to 30, inc.	7.58	6.14	5.76	7.42
31 to 35, inc.	5.26	4.76	4.56	4.54
Average	6.13	6.24	6.30	6.63
M.V.	.71	.82	1.11	1.19
Category av.	6.18		6.47	

each of the four arrangements. The average deviation of the individual judgments from the average position of each card have been calculated. It seems unnecessary to give this figure for each of the 35 cards, hence the total series of 35 has been divided into 7 sections of 5 cards each, and the average of the M.V.'s of each of these sections of 5 cards is given in the table.

In this table then we are dealing no longer with personal consistency but with the variabilty of a group of nine observers. Two facts of interest are disclosed by this table. The first is that, although the final averages of the variabilities under the four trials differ very little, such differences as are present point to *lower variability* for *similarity* judgments than for judgments of *difference*. Both the averages for similarity are lower than either of the averages for difference. There seems to be a slight tendency for the second trials to be more variable than the first, although the difference is small and not reliable. But such as it is, this difference is greater in the case of the difference series than in the case of the similarity series.

The second fact disclosed by the table is that with the arrangements for similarity the cards at the top of the series show smaller variability than those of the corresponding section at the bottom,—thus the first five tend to be less variable than the last five, the second less than the sixth, the third than the fifth. But with the arrangements for difference the reverse tends to be the case,—that is, the sections below the center of the series are less variable than the corresponding sections above the center. What this means then is this,—that whether judging in terms of *similarity* or in terms of *difference*, it is on the cards which are most like the standard that the judgments of the various members of the group of observers agree most closely.

Summing up the results of this table we may say that observers agree with each other more closely when judging *similarity* then when judging *difference*, and that in either case they agree more closely on the cards which are *more like* the standard than on those which are *more unlike* it.

The results of the two tables just discussed are further confirmed by those shown in Table III. One observer made arrangements of the cards for both similarity and difference 14 months after the original experiment, not having examined the cards in the meantime. These arrangements have been correlated with the similar arrangements of the original experiment. The correlation between the original and the later orders for similarity was .69. That for the original and the later order for difference was .62. But the correlation between the original order for similarity and the inversion of the later order for difference was only .36. That is to say, with an interval of over a year, personal consistency for *similarity* is somewhat higher than that for *difference*, and the difference between the one category and the inversion of the other is striking.

The final group average orders for the four arrangements have been correlated, and Table III. presents these coefficients also. They are all four extremely high, and the differences between them are so small as to afford no suggestions.

TABLE III

MISCELLANEOUS CORRELATIONS OF ARRANGEMENTS

Correlations of Final Group Average Orders:	
1st order for similarity, with second trial	.93
1st order for difference, with second trial	.95
1st order for similarity with reciprocal of 1st order for difference	.93
2d order for similarity with reciprocal of 2d order for difference	.91
Subject H. L. H., correlations of trials 14 months apart:	
1st resemblance with resemblance 14 months later	.69
1st difference with difference 14 months later	.62
Order for resemblance with reciprocal of order for difference secured 14 months later	.36

In the following pages are given the introspections secured from the nine chief observers whose results have been recorded, and also introspections from several others who were asked to make but one arrangement, some for similarity and others for difference. A discussion of the significance of these introspections will follow them.

INTROSPECTIONS

Resemblance

DeN.: The principal thing upon which my judgment was based was the general slant of the writing,—that is, the sample was in a hand slanting from right to left and the ones slanting in the same general direction looked more like it than the vertical or backward. Another thing was the formation of the capitals, especially of the letters P and C. Another factor was the space between the letters,—whether a word was all connected or whether it was broken.

Kup.: At first the actual combination of various types of handwriting, *e. g.*, slant, round, backhand, as evidenced in the type given as a model appealed to me and I was inclined to sort the cards according to this 'combination type.' Soon, however, the elements of character, of the personality in back of that type copy claimed my attention and this criterion established itself in my mind as a standard by which to judge the others. I characterized the type copy as having elements of rapidity, definiteness, free movement and no-waste-of-time. It seemed that of a decided, quick thinking person. According to such chracteristics I tried to arrange the cards given.

Hrt.: The first resemblance I thought of was that of slope, then the question as to whether the joinings between the letters were sharp or curved. Then I compared the relative height and depth of the letters, above and below the lines. Then I noticed endings of words, whether they ended abruptly or with a flourish. Methods of crossing t's and dotting i's were noticed and also methods of finishing y's and g's. The apparent ease of the writing always struck me,—whether it seemed to swing along easily or to be stiff and cramped. The size of the letters received little attention on the whole.

Rich.: My introspections are just about the same as when I arranged the cards for difference instead of resemblance, except that instead of looking to see how the cards differed in general appearance, placing, slant, color, etc., I looked for similarity in these respects.

Bar.: I was influenced primarily by regularity or irregularity of lines in the writing. If the whole seemed to be made up of lines going in all directions I was inclined to classify it as like the standard. If the whole presented an orderly appearance I did not consider it like the standard. I was influenced also by the width and prominence of the pen line—choosing first those that were darker and heavier, like the standard. Sometimes I found myself comparing only the one word 'Psychology' on the various cards,—then when I tried to see them all at once—the factor of regularity or irregularity was the strongest. Slant had some influence, but the judgment was much a matter of *general impression*, without any special factor so prominent. The ideas were mainly impressionistic,—I was guided more by a feeling of like or unlike than I was by any specific comparisons.

Str.: I first grouped the cards according to the position on them of the three lines of writing, then according to *uniformity*, regardless of the style or legibility, and finally, when the cards were very poor, according to legibility.

L. S. H.: I based my judgments of similarity to the standard on the shape of the letters and the slant of the writing.

H. L. H.: Began in terms of slant and judged on basis of slant, roundness of letters and general appearance of the card, until about two thirds of the way down. Then the slants were all reversed, the judgments seemed more difficult and the criterion was shifted to letter formation,—angles, tails of y's, capitals, becoming more important. On turning back to the start, after the first arrangement, these later factors asserted themselves, and I rearranged the first few cards, paying more attention to the smaller details than I had done before.

Cas.: The general character of the writing as a whole was the main basis for the arrangement. By that I mean the general size, boldness or fissiness and regularity. Next in importance was slant, and then the formation of the various letters.

Wund.: I judged first by the general character of the writing, then by the slant of the letters, the distance the letters were apart, and their general roundness. As I reviewed my first arrangement I made several changes according to the resemblance of the final letters of the different words, noting whether they turned up or down. I also watched for the ways in which the t's were crossed.

Lyo.: Personally I think I more or less unconsciously considered several factors, such as shade of ink, position on card, legibility, script, and size,—I said 'this or that card is *like* the standard' without forming the reason in words.

And.: First on the type of handwriting,—an extremely masculine type,—then on the slant of the letters and lastly on their form.

Hod.: I based my judgment chiefly on the general appearance and direction of the writing, whether it was slanting, upright or backhand. I took into consideration also the size of the writing, the spacing of the letters and the form of the letters themselves.

Wd.: In the first place I tried to pick out handwriting with the same general slant and carelessness and arrangement. Then I noticed the capitals and then the endings of the words, the spacing and the size of the letters although these latter I did not use very much. The general features seemed more important to me than the smaller details.

Difference

DeN.: I paid more attention to the formation of independent letters than when I arranged the cards for resemblance. Used slant until about one third of the way through, then had to rely on minor details, and the task became harder.

Kup.: This arrangement was constantly harder than the previous one, because of my inclination to arrange as I had done last time when the order was that of resemblance. When instinctively I felt the great difference of a card I very often remembered that I had not placed it so low in the order for resemblance. I labored between two impulses,—one to be true to my previous judgments and the other to act honestly according to my present light. I think I succeeded in following the latter. I noticed as I had not done before, to so great an extent, the great resemblance of groups of cards. Very often they seemed to have been written by the same person, but with the intention to disguise his handwriting. In such cases I noticed the *details* of the penmanship and made my decision rest with such little points as the separation of letters in a word, the crossing of a t or the last stroke of the y. . . . Throughout the relation of resemblance was in the background of consciousness. I felt that it was involuntarily more a criterion than the standard of 'difference.' The problem seemed far more puzzling this time than last.

Hrt.: In ranking according to dissimilarity I did not think first of slope, as in the arrangement for resemblance, but rather of differences in endings of letters like, g, y, etc., and in beginnings of words after capitals.

Rich.: I first looked at the general type of writing, *i. e.*, the slant, the size of the letters and the blackness of the ink. After this more general survey I thought sometimes of the similarity of the formation of the letters and the capitals, but this was necessary only when the general survey did not show striking enough differences.

Bar.: First the general appearance of the writing in its suggestion of the character of the writer. The pattern seemed to express a type of individuality entirely different

from that expressed in the card which I placed on top. This is a question of general impression. For cards more nearly alike I think the strongest point was in the regularity or irregularity of the letters. Some seemed to be regular and according to some definite system, others, like the sample, seemed to be more or less hit-or-miss style. Another feature was the width of the pen line. Next came the question of slant, although this was not a very strong factor. The formation of the individual letters was also of small import, but the final letters of each word influenced me somewhat, also the capitals. The question of motor imagery seemed to be a determining factor, —I seemed unconsciously to wonder how differently one should go about it to write the various cards, and to think of the hand movements necessary to the writing. This was a very strong factor in judging those that were particularly dissimilar.

Str.: Judged by general conception of smoothness rather than by actual comparison of standard. This may have been due to the fact that I had just read Dearborn's article on 'The Discernment of Likeness and Unlikeness.' Found the judgment harder than that of similarity and laid more stress on details which went to make up general smoothness. Distasteful job, goes counter to normal mode of doing things. Tended for a while to think of similarity. Do not feel sure of my judgments.

L. S. H.: Felt less decided than when making judgments of resemblance. Judgments vaguer. Felt as though about to come down stairs backward, and thus a little uncertain of progress. Judgments based on slope, shape and size of the letters with some tendency to consider the 'maturity' of the writing.

H. L. H.: Began in terms of general slope and 'rapidity.' Felt rather in the air and soon found the criterion inadequate. Then adopted size for a while, then formation of separate letters, tendency to flourish, and way of ending y's, g's, and d's. In the last part the tendency to think in terms of resemblance was strong, because the cards resembled each other in slant of the letters. Had to use finer and finer details.

Wood.: I judged first on the form of the letters and the way in which they were made, then on the general direction,—vertical, slant, or backhand. Then the position of the words on the card, and finally such details as the crossing of the t's, the endings of the y's and the way the e's were made.

Gold.: My judgments were chiefly based on differences in slant, size, and heaviness, My first judgments were made by examining the writing as a whole, comparing one card with another. Later I studied the individual words and letters, comparing their shape, roundness or sharpness, whether connected or not, method of crossing t's, etc.

Read.: In deciding the differences in handwriting the first consideration was the general appearance. So long as the cards of decided vertical writing held out I went by that. I then noticed the differences in the formation of the letters and particularly the first and last letters of a line. Of course to some extent the general effect was still of influence.

Crand.: I first observed the general character of the writing. The standard seemed to me to be freely flowing, accustomed, and not particularly careful. I began selecting those cards which were most carefully and apparently most slowly written. and those which seemed to have been written with some difficulty. As the most striking cards were eliminated the process became more difficult and I paid more attention to the formation of individual letters.

Plum.: The factors considered were general neatness, angles and slant, size of the writing, arrangement of the lines on the cards, and the form of special letters such as the d and the C.

Two things are indicated with considerable clearness by

these introspective records. The first is the greater ease and naturalness which is felt to characterize the judgments of similarity. This is best revealed in the introspections made during arrangements for difference. Thus Kup. reports: "This arrangement (difference) was constantly harder than the previous one (similarity). . . . The problem seemed more puzzling this time." Str. records: "Found the judgment harder than that of similarity. . . . Distasteful job, goes counter to the normal mode of doing things. Tended for a while to think of similarity. Do not feel sure of my judgments." Similarly L. S. H. remarks: "Felt less decided than when making judgments of resemblance. Judgments vaguer. Felt as though about to come down stairs backwards, and thus a little uncertain of progress." H. L. H. reports: "Felt rather in the air, . . . found the criteria inadequate . . . tendency to think in terms of resemblance was strong."

The second fact is suggested by such statements as often occur when judging difference,—"I paid more attention to the formation of independent letters than when I arranged the cards for resemb'ance" (DeN.); or, "I noticed the details of penmanship and made my decision rest with such little points as the separation of letters . . . , the crossing of a 't' or the last stroke of a 'y'" (Kup.). Also "I did not think first of slope, as in the arrangement for resemblance, but rather of differences in endings of letters like 'g,' 'y,' etc., and in beginnings of words after capitals" (Hrt.). "Began in terms of general slope and rapidity . . . and soon found the criteria inadequate" (H. L. H.). "I judged first on the form of the letters and the way in which they were made" (Wood). The judgment of difference, that is to say, is largely or often based on the comparison of fine points and minor details.

The introspections for similarity, on the other hand, abound to a much greater degree in references to 'slope,' 'general slant,' 'character,' 'personality,' 'regularity,' 'uniformity regardless of the style or legibility,' 'general impression,' 'carelessness,' etc.—all of these factors of a large,

general, loosely defined and 'impressionistic' character. These differences in criteria tend to assert themselves without regard to the order in which the arrangements are made.

Summary

1. The personal consistency correlation of two arrangements on the basis of *similarity* is greater than that of two arrangements for *difference*, unless, by performance in the 'mixed order,' or by some other circumstance, both categories are brought explicitly together in the consciousness of the observer.[1]

2. Both the correlation of two orders for similarity and of two orders for difference are higher than the correlation of an order for similarity with the reciprocal of an order for difference.

3. With repetition, adaptation and familiarity with the material the two categories tend to approximate each other and the direct order to agree more closely with the indirect order.

4. The variability among a group of observers is less for similarity than for difference.

[1] A possible objection at this point might be that the differences in the two arrangements were perhaps due to the fact that the two arrangements began with different cards (the similar end of the series in one case and the unlike end in the other), rather than to a real influence of the form of the judgment. A test of this would be afforded by observers who should arrange the cards in terms of *similarity* (beginning with the most similar) and also in terms of *difference* (beginning with the least different instead of with the most different). When such an experiment was tried with three observers, all three showed clearly that, in the attempt to reason out what might be meant by "least different," the two categories were at once brought explicitly together in the consciousness of the observer. Since logically the 'most similar' is the 'least different,' the arrangement then proceeded in terms of similarity, even when the instructions were in terms of difference.

The apparent objection is not a real one. The observer has all the cards before him. Whatever cards are judged to be 'least similar,' he may leave till the latter part of the series, if he chooses, when judging similarity. When judging difference, whatever cards he judges to be most different may be at once selected. The whole matter is in the observer's own hands. And the significant thing is that the cards which are left to the end of series, when judging similarity, are not precisely the ones selected for the earlier part of the series when judging difference.

Furthermore, if the result were only a consequence of inverting the series, the two orders for difference should correlate as closely as, and show no greater variability than, the two orders for similarity. Neither of these conditions is realized. The difference is then not merely the result of inverted arrangements.

5. Whether the judgment is expressed in terms of similarity or in terms of difference it is on the cards which are most like the standard that the group agrees most closely.

6. When arrangements are made 14 months apart, the same relations are disclosed,—personal consistency for judgments of similarity is greater than that for judgments of difference, and the discrepancy between the direct order and the indirect order secured by inverting the arrangement under the opposite category is striking.

7. Introspection suggests the greater 'ease' and 'naturalness' and 'confidence' of the judgments of similarity.

8. Introspections also show a different distribution of criteria in the two categories. Judgments of similarity tend to be based on grosser and more general criteria,—such as character, slope, ease, rapidity, etc.; the judgment tends to be 'impressionistic.' In judging difference more attention is paid to the finer details of form, size, arrangement and separation of letters.

9. Judgments of similarity and of difference are not merely two forms of expression of one and the same intellectual act. Judgments within each type or category involve each its own peculiar psychological processes and criteria. The 'most similar' is not, by virtue of that fact, the 'least different,' nor is the 'least similar' identical with the 'most different'. Of the two categories, similarity seems to be the more fundamental, natural, easy and self-consistent, whether a single individual or a group of observers is concerned.

10. In these respects judgments of similarity and of difference behave in the same way as do judgments of other logically opposite qualities (such as preference and dislike, intelligence and stupidity) which involve, in the beginning of such an experiment,[1] psychological processes and criteria which are not identical, but which move to a common plane as the experiment proceeds or is repeated.

[1] See 'The Influence of Form and Category on the Outcome of Judgment,' by Margaret Hart Strong and H. L. Hollingworth. *Jour. of Phil., Psychol., and Sci. Meth.*, September 12, 1912.

STUDIES IN ASSOCIATION AND INHIBITION[1]

BY JOHN F. SHEPARD AND H. M. FOGELSONGER

This investigation grew out of an attempt which was begun several years ago by one of the writers (Shepard) to analyze more fully, introspectively and objectively, the processes involved in the simple reaction experiment. It is not the intention to report that work in detail here; in fact, further experiments will be performed before the results are published. But a brief review of the introspections obtained would perhaps make the idea of the present study clearer.

In common with Ach and others, it was found that there is typically consciousness of both stimulus and reaction, and the only differences are differences in detail of representation. But these two stand in a sort of double relation to each other. One side of this relation is emphasized when the attention has been too much absorbed in the representation of the stimulus, the subject is not prepared to act and has to get ready in part after the stimulus comes. One appreciates especially then that the stimulus is associated with the act and tends to call it out; there is a conscious push toward the act. This is the only condition in which there was anything that could be called a distinguishable will impulse.

The other side of the relation is brought out well when the experiment is arranged as follows: The subject is told that he may react to whatever stimulus comes. But he is asked in each case to expect a certain definite color or sound, to imagine this in as much detail as possible, and, in short, to be primarily ready to react to this particular stimulus. Only those results are used in which the subsequent introspection shows that the subject definitely expected the given stimulus. To make this possible, the announced stimulus is usually given. But from time to time a color or sound more or less different from the expected one is used, or the expected stimu-

[1] From the psychological laboratory of the University of Michigan.

lus is given in combination with another. The results in these cases are of importance. The subjects report a 'sudden checking all over,' or that 'the movement was not so smooth,' 'a sense of goneness,' 'the whole process seems to stop and turn back on itself,' 'confusion,' 'hesitation,' and other similar expressions. The action is carried out against resistance. In some cases the action does not take place at all. Usually there is merely a consciousness of hesitation; if there is much delay the subject remembers that he is to react to every stimulus, and does so. The amount of interference with the objective reaction time probably depends upon whether the act demanded is short and simple or longer and more complicated. The short movement may be completed by the shock of surprise or by the initial tendency to movement caused by a stimulus. The important fact in the present connection is that there seems to be a definite positive interference or blocking as a result of a variation in the stimulus. This interference is not a mere lack of a sufficient stimulus, and it is not the effect of a preparation to withhold reaction to all stimuli but one. It is a sort of automatic result of the plan to react to a definite stimulus.

According to these introspections, the preparation of the subject for the reaction may be analyzed as follows. Both stimulus and action are present together in consciousness, and they are parts of a whole. This wholeness finds its basis in the fact that the stimulus, or representation of the stimulus, tends to arouse the act by association, and yet that an incompleteness or variation of the stimulus inhibits the act. The coming of the correct stimulus simply seems to fit into this preparation, to be really a completion of the stimulus, and so remove the inhibition and allow the process to carry itself out. If this analysis is true, we have an inhibition which it would be difficult or impossible to bring under the concept of drainage. No connection or path has been formed which would rival that already 'sub-excited' between the representation of the stimulus and the reaction. It at least becomes desirable to investigate the possibility and laws of inhibitions where there is no division of established connec-

tions, or where the connections established work toward the same end.

In the experiments[1] for this purpose, nonsense syllables were used, and the method was similar to the common method of successes in memory investigations. The memory apparatus used in the first experiments was the Wirth form made by Zimmermann. In the later tests this was somewhat modified. The main change was in the cylinder which carries the material to be learned and tested. In the original machine, the electric contacts at the inner end of the cylinder are stationary. For the tests after learning in these experiments it was necessary in some cases to make the electrical connection on every second space, in other cases to make it on every third space; and it was necessary to change quickly from one arrangement to the other. Accordingly, a new cylinder was made in which each contact is attached to a metal slide. This slide runs lengthwise in the cylinder just under the surface, and terminates in a catch at the outer edge. By this means each contact can be quickly and easily moved through a sufficient distance to make or break the circuit. The Hipp chronoscope prepared for a make-and-break circuit served to measure the time between the exposure of the test syllable and the recall. In a part of the tests a voice key and relay was used as the reaction instrument. In a part the lip key was used. In some of the earlier tests the reactions were taken with an instrument designed to enable the movement of the larynx to break the circuit. The same apparatus was always used with any given subject.

In the first series of experiments, the syllables were arranged in pairs. In each experiment two sets of syllables with seven pairs in each set were used. All the syllables were different except that three of the second syllables (second in the pair) were common to the two sets. Pairs with a common second syllable did not usually occupy the same position

[1] Mr. Fogelsonger took part in the first, second and third series of tests. The subjects for these were Mr. McAlpine, Mrs. Fogelsonger, and Mr. Carpenter. We acknowledge our indebtedness to them. I have done the remainder of the work alone. I am especially indebted to Dr. Adams, Miss Perkins, and Mr. Winter, who acted as subjects for the greater part of the work since.—J. F. S.

in the two sets, and the position in which such pairs were placed was varied from experiment to experiment. In learning, the syllables were exposed regularly, one pair at a time, in the memory apparatus, and the subject read them aloud. The first set was repeated twenty-five times, then there was a wait of twenty-five minutes, and this was followed by twenty-five repetitions of the second set. A day later both sets were again given the same number of repetitions, except that the second set was now used first. A further wait of one hour was then followed by the test.

In testing, the first syllables of the pairs of both sets were shown either alone or in different combinations. Altogether seven arrangements were used in the work of the first series. If single letters are used to indicate syllables (each letter stands for a distinct syllable), these seven forms may be explained as follows. No. 1, the subject has learned *a–b*, *a* is exposed in the test; No. 2, the subject has learned *c–d* and *e–d*, in testing, *c* and *e* are exposed in succession; No. 3, *f–g* and *h–g* have been learned, *f* and *h* are exposed simultaneously in the test (*f* printed over *h*); No. 4, *i–j* and *k–l* have been learned, in testing, *i* and *k* are shown in succession; No. 5, similar to No. 4, except that the test syllables are shown simultaneously; No. 6, *m–n* learned, *m* and *x* (a new syllable) shown in succession; No. 7, similar to No. 6, except test syllables shown simultaneously. When two syllables were shown in succession, it was planned to allow the first to catch the attention, and if possible begin to recall its association sufficiently so that the subject would feel on the verge of getting something at least, but to have the second replace the first before the association process could be completed. It was arranged so that the clock was not started until the second syllable appeared. In order that the subject could in no case anticipate what variety of test would be given next, these seven types of arrangement were used in a mixed order, which was varied from experiment to experiment.

On account of the easy confusion of half-learned similar syllables of different groups that are crowded too close together, it seemed practical to allow a few days to elapse be-

tween the separate experiments, especially as the object was to study the interferences or inhibitions. On the average only one or two examples of each of the seven forms could be given in a single experiment, and a few of these must, of course, be thrown out because of distraction of the subject, failure of the apparatus, or other such reason. The results, therefore, accumulated very slowly, but it was thought that a smaller number of clear cases would be of more value than a larger number of less certain ones. The consistency of the results probably justified this expectation. A similar remark would apply to each of the series of experiments to be described.

The results of the first series are brought together in Table I. The table is divided into three parts. In the first part the average time (expressed in sigma) of the correct associations is given for each subject and each form of test. In the parenthesis under each time average is shown the number of results for the given form and subject which could be made use of. In the second part of the table, the average time of No. 1 for each subject is expressed as 100 and the time of the other forms are reduced to corresponding ratios. In the third part the percentage of correct associations is given.

TABLE I

	Subject	No. 1	No. 2	No. 3	No. 4	No. 5	No. 6	No. 7
Average time	Mc.	1,122 (49)	2,139 (17)	2,594 (21)	2,512 (26)	2,873 (16)	1,709 (12)	2,609 (11)
	F.	1,288 (47)	2,154 (19)	3,204 (21)	3,053 (32)	3,087 (23)	2,681 (12)	3,695 (14)
	C.	855 (23)	1,395 (15)	1,955 (16)	2,520 (14)	—	—	—
Relative time	Mc.	100	191	231	224	256	152	232
	F.	100	167	249	237	239	208	287
	C.	100	163	229	296	—	—	—
Per cent. correct	Mc.	78	88	55	69	75	58	45
	F.	83	79	71	66	78	42	43
	C.	83	80	69	64	—	—	—

In No. 4 there were more associations with *i* (the first shown in the test) than with *k* (second shown in the test). In No. 5 they were about equally divided.

Only a few results were taken from subject C., and these

were of small value since the cases necessary for comparison were not obtained.

Only two of the subjects, Mc. and F., served for the second series of experiments. As before, the syllables were arranged in two sets for each experiment, with seven groups in each set. A part of the groups consisted of two syllables as in the first series, but in a greater part, two syllables, one placed over the other, were substituted for the first syllable of the pair, thus giving a group of three. In learning a three-syllable group, the subject repeated the upper and then the lower of the two syllables on the left, and thirdly the syllable on the right. All syllables were different except that a part of the third syllables in three-syllable groups were common to the two sets. The position and number of different types of groups in the set varied from experiment to experiment. The method in learning was similar to that of the first series.

Three arrangements were used in testing. Using letters to indicate syllables as before, these forms may be stated as follows. No. 1, the subject has learned *a–b*, *a* is exposed in the test; No. 2, the subject has learned *c/d–e* (*c* over *d*), *c/d* (*c* over *d*, both in view) is exposed in the test; No. 3, *f/g–h* and *i/j–h* (*f* over *g* and *i* over *j*) have been learned, *f/j* (*f* over *j*) is exposed in testing. The results of this series are shown in Table II. which is drawn on a plan like that of Table I.

TABLE II

	Subject	No. 1	No. 2	No. 3
Average time	Mc.	784 (16)	1,493 (16)	1,697 (16)
	F.	1,496 (16)	2,513 (19)	3,480 (22)
Relative time	Mc.	100	199	228
	F.	100	168	232
Per cent. correct	Mc.	94	88	88
	F.	81	79	73

The third series consisted of a few tests carried out on one subject, F. Where three syllables were given in a group in learning, they were printed in a horizontal row instead of

having the first over the second. Where two syllables were exposed at once in testing, they were also arranged in a horizontal row. The different types of test were as follows. No. 1, *a–b* is learned, *a* is shown in the test; No. 2, *c–d–e* is learned, *c–d* exposed in testing; No. 3, *f–g–h* and *i–j–h* learned, *f–j* shown in testing; No. 4, *k–l–m* and *n–o–p* learned, *k–o* exposed in the test.

The results are given in Table III. It may be added that the results under each type were fairly uniform; the mean variation was comparatively small. For this reason the averages mean something in spite of the small number of results.

TABLE III

	Subject	No. 1	No. 2	No. 3	No. 4
Average time	F.	1,392 (11)	1,957 (9)	5,279 (11)	5,922 (8)
Relative time	F.	100	140	379	424
Per cent. correct	F.	90	78	90	38

It will be seen that in the first series, the average time of No. 2 is greater than that of No. 1. No. 3 is greater than No. 2. No 7 is greater than No. 6. No. 5 is only slightly greater than No. 4. No. 3, No. 5 and No. 7 are sufficiently near together to be placed in the same class. No. 4 is definitely greater than No. 2. In comparing the first series and the second series, it is seen that Mc. improved markedly with practice as shown by the simple association time. It is doubtful whether this practice effect would be transferred so fully to the learning of more complicated groups like No. 2 of the second series; but even if we allow equal practice effects throughout, and make the comparison of series simply on the scale of ratios, we find that No. 3 of the first series is greater than No. 2 of the second series. No. 3 of the second series may be placed in the same class as Nos. 3, 5 and 7 of the first series.

Groups of syllables arranged in a row as in the third series probably have more of a tendency to be thought of as a unity both in learning and testing, since there is not so much chance to connect primarily the first of the doubles with the

syllable to be recalled. This would seem to be an advantage for those tested as learned, a disadvantage for those tested in different combinations. Accordingly we find that No. 2 of the third series is relatively less than No. 2 of the second series, and Nos. 3 and 4 of the third series are markedly greater than No. 3 of the second series.

Following the third series, a few experiments were carried out on five other subjects with more detailed introspections. The average times agree in relative size with those above. The introspections emphasize the necessity of further care in order to evaluate the results. Whenever the syllables were tested in a combination different from that in which they were learned, particularly when learned as single pairs and tested by exposing two syllables, and the subject attended to the test syllables as a unity, there was a consciousness of confusion, obstruction, being bothered, etc., and the time was long. If the subject for any reason had attended to a separable element only, and had simply built up the association from it, there was no such trouble. When a three-syllable group had been learned and two of the syllables were shown to recall the third, if the subject attended to the two as a unity, the association came naturally; if he attended to only one of the syllables and tried to obtain an association from it, he found himself blocked and unable to recall. It is therefore an error to average all results obtained under given external conditions as though they were equal. The fourth and fifth series were designed to meet this criticism.

In the fourth series, four sets of syllables were used in each experiment. Each set contained five groups. In set 1, set 2 and set 4 each group consisted of a pair of syllables. Set 3 was made up of three-syllable groups, the first two syllables of each group being printed one over the other as in No. 2 of the second series. The second syllables of the pairs of set 4 were the same as the second syllables of the pairs of set 2 except that they were arranged in a different order. All other syllables were different. In learning, the subject repeated set 1 fifteen times, waited eight minutes, repeated set 2 fifteen times, waited eight minutes, and so on through

sets 3 and 4. After twenty-four hours the same process was gone through again. It is important to state that according to the reports given afterwards, there were practically no cases in which the subject in learning set 2 or set 4 recalled the corresponding group of the other set. The test began one hour after the last repetition.

In testing, the subject was shown the first syllables of the pairs of set 1, the pairs of syllables printed one over the other from the groups of set 3, and similar pairs (one syllable printed over the other) composed of syllables having the same association in sets 2 and 4. We have, therefore, the following three types: No. 1, *a–b* learned, *a* exposed in the test; No. 2, *c*/*d–e* (*c* over *d*) learned, *c*/*d* (*c* over *d*) given in the test; No. 3, *f–g* and *h–g* learned, *f*/*h* (*f* over *h*) and *h*/*f* (*h* over *f*) shown in testing. Both *f*/*h* and *h*/*f* could usually be used some distance apart without the subject recognizing, at least until afterward, that he had previously seen the combination. Those few cases which he did so recognize were thrown out. The extra repetition from the first test would rather emphasize than interfere with our conclusions. The different types were mixed in irregular order in the test, so that the subject could not anticipate what would be exposed. With this arrangement the exposed syllables were usually immediately read and thought of together as a unity, but sometimes the subject apparently expected a No. 3 case and attended to either No. 2 or No. 3 as syllables to be taken separately, and so noticed only one syllable at first. After each individual reaction, an introspection was taken as accurately as possible. The subject was asked to state as completely as possible the process, from the exposure of the test syllable or syllables to the final response. In fact, the introspections were especially emphasized in this and the following series. The test periods were consequently very much protracted. On account of the length of time involved, this form of experiment was repeated only three times, giving fifteen cases of Nos. 1 and 2 and thirty cases of No. 3 for each subject. The results, however, are quite uniform and may therefore be given a relatively great weight.

The subjects in this and the following series were Dr. Adams, Miss Perkins and Mr. Winter. All had had extended experience in other investigations with nonsense syllable material.

The results are summarized in Table IV. In making the table, except for the per cent. of correct recalls, only those cases of No. 2 and No. 3 are considered in which the subject attended to both test syllables. The table shows for each subject and type, the total per cent. of correct associations, the average reaction time of those correct, the per cent. of correct associations that gave reaction times greater than 3,500 sigma, and the per cent. of correct reactions that are less than 2,000 sigma. In several cases P. felt that she had not learned set 3 so well for some reason, and this probably helped to lengthen the time of No. 2 in her case. Also the mere reading and grasping of two syllables in testing requires a longer time than one syllable. Yet No. 2 is shorter for two subjects than No. 1.

TABLE IV

Subject		No. 1	No. 2	No. 3
P.	Average time	2,050	4,113	5,303
	Per cent. >3,500	7	40	64
	Per cent. <2,000	62	30	0
	Per cent. correct	100	93	93
W.	Average time	2,992	2,794	8,841
	Per cent. >3,500	36	12	75
	Per cent. <2,000	27	0	0
	Per cent. correct	87	67	78
A.	Average time	3,565	2,048	10,548
	Per cent. >3,500	40	9	100
	Per cent. <2,000	40	60	0
	Per cent. correct	67	80	70

With type No. 3, there were 6 cases in which W. attended immediately to one of the syllables separately, and disregarded the other, giving an average time of 2,918. Seven similar cases for A. gave an average of 2,510, and seven such cases for P. gave an average of 1,953. Two cases in which W. attended to No. 2 as singles at first gave 3,936 and 6,940.

In the fifth series as in the fourth series, four sets of five

groups each were used. The first set was composed of three-syllable groups. In learning it, the first syllable of a group was thrown into view alone at the left end of the opening, then after a brief pause (only about long enough to read the syllable clearly), this was thrown out of view and the other two syllables printed in a row were thrown in. About one second was allowed in which to read these, when they were replaced by a blank space, and a longer pause of about two seconds was given before another group. Since the release was worked by hand, these pauses were only approximate. The other sets were made up of pairs of syllables. Sets 2 and 4 had common second syllables as in the same sets of the fourth series. All other syllables were different.

The number of repetitions and the waits were the same as in the fourth series. In testing, the first two syllables of set 1 were shown in succession as they had been seen in learning. Likewise, each two syllables associated with the same common second syllable in sets 2 and 4 were shown in a similar succession. Entirely new syllables were combined with the first syllables of the pairs of set 3, and the resultant pairs were shown in succession, the new syllable first followed by the syllable which had been used in learning. The three types are then as follows: No. 1, *a*—followed by *b–c* given in learning, *a* and *b* exposed in succession to test; No. 2, *d–e* learned, *x* and *d* given in succession for test; No. 3, *f–g* and *h–g* learned, *f* and *h* in succession, and *h* and *f* in succession used in testing. When the syllables were used again in reverse order in No. 3, if the subject recognized the combination it was thrown out. When syllables are shown in succession in the test, the clock is not started until the second comes into view. An introspection was taken as thoroughly as possible after each reaction. As in the fourth series, only a few experiments were carried out, but the results are very consistent and the introspections were of especial value.

The results are summarized in Table V. In the table, except for the per cent. correct, only those results are considered in which the subject attended to both syllables in the succession as belonging together in a unity. Where the

attention was caught by one syllable alone, the responses were separated and considered as of another class. The table shows for each subject and type of test, the total per cent. of correct responses, the average time of correct reactions, the per cent. of correct reactions that are more than 3,500 sigma, and the same less than 1,500 sigma. A. nearly always disregarded the first syllable with No. 2 cases, and attended only

TABLE V

Subject		No. 1	No. 2	No. 3
P.	Average time	1,364	5,013	3,211
	Per cent. > 3,500	0	80	38
	Per cent. < 1,500	66	0	6
	Per cent. correct	90	80	95
W.	Average time	1,755	5,051	3,100
	Per cent. > 3,500	0	60	45
	Per cent. < 1,500	50	20	11
	Per cent. correct	90	80	88
A.	Average time	2,939	—	3,564
	Per cent. > 3,500	33	—	62
	Per cent. < 1,500	33	—	12
	Per cent. correct	100	—	80

to the second, or else failed to get the association, so these spaces are left blank. In only two successful cases did he attend to both, and with these the times were above 10,000 sigma.

Two cases each in which P., W. and A. tried No. 1 separately at first, averaged 3,075, 2,088, and 5,289. Three cases for P., two cases for W. and five cases for A. in which the first syllable of No. 2 was disregarded at once, and the subject attended to the second as a single syllable, averaged 2,057, 1,546, and 2,910. Of cases of No. 3 in which the subject attended to one syllable only (usually the first) and did not notice the other, two for P. gave 1,172, four for W. gave 1,155, and two for A. gave 727.

If, before we study the introspective reports in detail, we consider the results so far outlined, we find evidence of a definite inhibition which cannot be brought under the ordinary interpretation of generative and effectual inhibition and which cannot be explained neurologically as a division of energy, or

drainage. Suppose the syllables have been connected separately with the same additional syllable, and then the two test syllables are shown in succession, so that the first may be grasped before the second comes into view, and the time is measured from the appearance of the second. We should expect the first to at least partly rearouse the syllable to be recalled, and the subject indeed is often conscious of a more or less complete suggestion of the association when the second syllable appears. This second syllable should then complete the association decidedly more quickly than it otherwise would. We have two paths converging toward the same end result, one of them is started into action first, and the time is measured only from the beginning of excitation of the second. They should reinforce each other strongly. Even aside from this, if we consider merely that we have two paths of which either may be excited independently, the more rapid of the two would respond, we should have a selection of the quicker of each pair of associations, which would give a shorter average reaction time. But as a matter of fact, we find that the double test (two syllables in succession) gives a longer average time and a greater per cent. of excessively long times (more than 3,500 sigma) than the simple association, with all subjects except A., and with A. there is a greater per cent. of the exceptionally long times. Furthermore, this is true in spite of the circumstance that some cases which were probably real reinforcement occurred in the double tests. If the subject gets some suggestion from the first and then has time to dismiss the first definitely and turn his attention to the second when it comes, there is probably facilitation rather than hindrance. These cases were usually counted among those in which the subject attended to both syllables, which shortened the average time.

If we compare such cases where the connections are learned separately and tested as doubles shown in succession (as No. 3 of the fifth series), with other cases where three-syllable groups were learned by exposure in succession and tested as doubles shown in succession (as in No. 1 of the fifth series), the difference in time is found to be still greater (Table V.).

In the first case there are two relatively deep paths converging toward the same response; in the second case, according to the usual explanation, there would be some connection established between the first syllable and the response, but the main paths would be from the first syllable to the second and from this to the response. If it were a simple mechanical process, we should expect the first case to be the more rapid. But whether measured by the average time, the per cent. of very long times or the per cent. of very short times, those tested as learned have a decided advantage. In the latter case, the greater amount of process leading to the recall has a sort of cumulative effect and arouses the response more quickly.

We reach a similar result if we study the cases which have been learned as two separate pairs with a common second syllable, and tested by exposing the first syllables of the pairs simultaneously. Here again we have two converging paths leading to the same response, which should, according to the simple mechanical conception, reinforce each other. Aside from reinforcement, we should expect the quicker of the two to control the association time and give us a shorter average time than if only one syllable were shown at a time. On the other hand, if the connections have been learned as three-syllable groups, with the same number of repetitions as each pair in the previous case received, according to the ordinary conception there would be some connection of the first syllable to the response, but the main discharge and the main path would be from the first syllable to the second and from this to the response. There is not the same convergence effect, or the same double possibility of recall, of which the quicker may work. We should expect then that those learned as simple associations, and tested as doubles shown simultaneously, would average at least as quick as the other cases. We find as a matter of fact that the average times and per cents of large times are markedly greater, and the per cents of small times are much less than either the simple associations or those cases learned as three-syllable groups and tested by exposing the first two syllables together. In-

deed we may go farther and say that the times of these separates tested as doubles must probably be placed in the same class with those cases in which the two syllables shown are associated, not with the same, but with different and conflicting syllables.

Likewise the times were very long when, in cases learned as three-syllable groups, the subject tried to attend to only one of the two syllables shown in testing. The same is true when parts of two triple groups were used to make a double group in testing, as in No. 3 of the second series and No. 3 of the third series.

When a pair has been learned and a new syllable is added to the first syllable in testing, as in No. 2 of the fifth series, it would seem that there ought to be at least as much reinforcement as drainage, and no effect upon the time. On the contrary, there is a prominent interference.

It seems impossible to understand these results except by the assumption of a form of inhibition of one association by another, which cannot be brought under the ideas of drainage or division of energy. This means that an association cannot be explained as a mere path of lowered resistance, a series of synapses of lowered resistance, which is the usual statement. An association involves other processes which prevent any other stimulus from using the same neurones at the same time, which check it if there is any variation or incompleteness in the recalling process, and which block any other association that is tending to operate at the same time, even though both will lead to the same end result.

We need not attempt to decide more definitely what is the nature of these processes, these inhibitions. At present we do not know. But we may point out the similarity of the inhibitions involved in these experiments to those demonstrated by Sherrington[1] for the reflexes of the cord. Sherrington shows that several reflexes may involve the same final common path, but each exciting it in a way different from the others. The excitations of two such reflexes, if they occur at the same time, interfere with each other. Such an inhibi-

[1] Sherrington, 'The Integrative Action of the Nervous System,' Ch. IV.

tion cannot be adequately explained by drainage. And we find that whenever one acquires an association, similar inhibitions are set up within the cortex. Possibly in time we may show more nearly the laws of the process here than in the case of reflexes, for we can control the conditions in forming and grouping acquired associations. In any case, since associations enter into all higher mental processes, such inhibitions must have quite general bearings. We shall try to bring out in some measure three instances of such general relations, and the first is suggested by a more detailed study of the introspections.

The introspections were usually reported in terms of the specific nonsense syllables concerned. In quoting from them, however, we shall in most cases replace the specific syllable by its number in the group. In dealing with the fourth series in which two syllables were shown printed the one over the other in the test, the one printed above will be spoken of as the first syllable, the one printed below as the second. The syllable to be recalled will be spoken of as either the association or the response. In the fifth series, of course, the first syllable is that one shown first, the second is that one which came into view later. The following are typical introspections given with No. 3 of the fourth series (learned as separates, tested as doubles). "Was confused. Kept going back and forth. Felt a sort of effort. The second got it." "Alternated from one to the other. Seemed bottled up. The first finally recalled it." "The two seemed to try to recall different associations." "First noticed both and found they didn't go together. Then got the response with the last." "Glanced at the two as a unit at first but didn't get anything. Then attended to the second and got the response and afterward saw it went with the first also." "Tried to get the response from them as a unity at first. Then noticed the second and it slipped off into the response." "First thought of them together and got nothing. Then alternated from one to the other several times. Everything seemed rushing in at once; confusion which was altogether different from the mere searching with a slow response when

they really seemed to fit together. Finally the first syllable got it." "Another bamfoozle. First tried together and failed and then went from one to the other in a confused way. Awful feeling. The second one got it." "Tried as a unit at first. Soon saw this would not work. Then alternated from one to the other and got the response with the second, then compared with the first before spoke." "I thought they belonged together. After had spoken noticed they didn't go together and got the correct response from the first." (The subject had given a wrong reply at first.) "Recognized at once that they didn't go together. Thought of the first, then the second and got a suggestion, then the first and got the response." "Recognized they were separate. Got the response with the first and didn't notice the second until afterward." "The first got the response and the second was not in consciousness at all." "Came from the first entirely and didn't notice the second at all." "Happened to see the second first, and got the response at once from it without noticing first."

Such reports as the following were given with No. 2 of the fourth series. "The two were in mind as a unit and recalled the response." "Saw them as a unit and got the association as a simple recall." "Saw them as a unity and recalled as a direct association." "Got it with a rhythm. No confusion." "Didn't know it well. Kept repeating the two to get the third by the rhythm, but it wouldn't come at first. Was slow." "Tried to recall as separate at first. Tried the first, got nothing. Tried the second, then saw they went together and got the response at once." "Tried them singly and failed to get anything. Then tried them together, saw they belonged together, and got the response."

For No. 1 of the fifth series, we have introspections like the following. "Normal. Learned it that way. Couldn't have gotten it with the first alone, had to see the second." "Recognized they went together at once. As soon as saw the first syllable, a suggestion of the whole came. Got the response after mentally reading over." "First impression was that they were separate. Then saw that they were

together and got the response." "Tried the first alone and couldn't get anything. Saw the second, knew they were a unity, and the response followed at once." "Didn't recognize the first immediately. Saw the second, knew they went together, and got the response at once."

No. 2 of the fifth series shows characteristic reports as follows. "Tried the first and got nothing. Tried them as a unity and that didn't work. Tried each separately and got the response with the second. I think I would have got it at once if the first syllable hadn't bothered." "Read it over and over again and couldn't think of anything with the two together or with the first. Then the second got it." "Couldn't get anything with the first and tried as a unity and that didn't work. Only after centered on the second that got it." "They wouldn't work together. Looked at the first and gave it up. Looked at the second and for moment when looking at second with first still partly in mind, there seemed to be a clamoring of more than one response to get in. Then got the response from the second and this confusion disappeared." "Saw the first but it left my mind as soon as I saw the second because I knew immediately the association for the second." "When saw the first, thought that one learned together was coming. Then when saw second, knew response at once, and the first simply disappeared immediately." "Did not feel like would get anything from the first and it entirely disappeared at once as the second came. The second brought the response with it."

Lastly, we find the following typical of No. 3 of the fifth series. "First tried to connect them into a combination and this didn't work. Then got it from the second. I think it should have been most familiar with the first, if I had attended to that alone properly." "Read it over and over and didn't know whether it was a unity or not. Confusion. Then got response from the first and forgot the other." "Recognized them as being separate. Alternated. Felt with each that I was just about to get the response, and then would stupidly change to the other and lose it all." "Recognized not together. Alternated rapidly from one to the

other. Got the response with the first. Could have got it more quickly if it had been only one to put attention on." "Almost got the response from the first, when the second appeared and drove it all out. Then in turn recalled it from second." "Saw they were separate. The first gave a suggestion. The second stopped the progress; then went back to first and got the response." "Practically got response at once from the first. Read the second and it all left me. Recalled first and got response from it." "Got it from the first before really saw second, and didn't pay any attention to the second." "Got the response from the first and spoke before the second was in mind. Second was in eye but not in mind."

We see that the subject can tell introspectively what process recalls the association; with the working of the association the recalling process practically always stands out clearly in attention. The association will not work effectively when one attends to the syllables in a different combination from that in which they were learned. Sometimes attention to a syllable and the association seem to come together. Sometimes the attention is absorbed in reading the syllables before the association is aroused. In many cases the attention is absorbed in an association which is being aroused by one of the syllables, and the other is not noticed at all, it is 'in eye but not in mind.' More often under the influence of the previous definite expectation to attend to the two syllables, which is emphasized by the occurrence of those that are to be taken as a unity, the subject turns his attention to the second syllable even when he has the association more or less complete from the first syllable. The result is that the whole process is blocked and the association driven out. When a new syllable precedes one that has been learned in some connection, the new syllable very often entirely disappears as the second comes into view, and the second at the same time recalls its association in nearly normal time. There is seldom any continued alternation in these cases. Under all conditions the inhibition process does not seem to be a mere checking, but one factor is simply pushed aside

and a definite association works and controls the field. In fact all the results can be most completely interpreted if we make the attention and the association-inhibition process identical. The unit of attention is not an isolated sensory process or the activity of a set of sensory cells, nor is it an isolated motor process or activity of certain motor cells; it is rather an association process, the passage of activity over a group of synapses changed as in the formation of associations with the accompanying inhibitions. Such an association as the unit of attention may be either from sensory to sensory or sensory to motor probably. Clearness consists in the fact that any definite association will, at least for the moment, set aside all others, inhibit and refuse to mix with all others, and hold itself relatively dominant and distinct.

If we turn back to the analysis of the process of the plan as shown in the simple reaction experiment, we find another of the more general bearings of inhibition. We found there that any incompleteness or variation of one stage probably inhibits the next. We find in the experiments that an association involves other processes that check it if there is any variation in the recalling process.

Lastly, a third general consequence of the inhibition side of all associations is suggested by experiments now in progress. By way of emphasis of the idea that inhibitions play a greater part in mental processes than is usually assigned to them, we may illustrate the suggestion by a single experiment carried out on the three subjects who served in the fourth and fifth series. Fifteen groups of three each from set 3 of the fourth series were rearranged into three sets of five groups each. The subject repeated each set fifteen times with seven minute waits between sets. After a wait of one hour they were tested. In the test, the first two syllables of five of the groups were shown as learned; in five of the groups some single letter was replaced by a new letter; and in five, two letters, either adjacent or not, were replaced by new letters. The subject was asked to recall an association as quickly as possible, but not to give up if he did not succeed at once. So few results have very little statistical value, but it may be stated that

the average time for those tested as learned, those with one letter changed, and those with two letters changed, were for W. 2,463, 4,643 and 9,740 respectively. For A. they were 1,693, 9,375 and 15,078. For P. we have 2,667, 5,920 and 11,614. The following are types of the introspections. "Don't know how I got it. Was enough alike that it recalled the old association, but am not sure whether it is right." "Recognized everything as right except the first letter and didn't even notice what this was. It simply disappeared as wrong. Then got the association." Fas/Yan—Gej had been learned and Fow/Yan was shown in the test. The subject said, "Recognized second as correct. Then saw F—correct. —ow disappeared and had Fas and then got the association at once from reading the whole together." Yif/Sic—Coh was learned, Lif/Jic was given in the test. Introspection: "Expected the first to be right and yet couldn't believe I had seen it. Then all of a sudden the first letters of each dropped off as though they were not there, and I recognized the second two in each as like those I had had, and searched for the first. These came quickly and got the association at once. Didn't think of L— and J— after they dropped away."

In these and other variations of the experiments, it is found that the subject sometimes recalls the association without definitely distinguishing the likenesses and differences, and then is practically always uncertain of the result. Usually a sub-group of characters seems to separate itself, the others disappear as inconsistent, and these hang together as the center for recall of the larger group. Without going into details it is desired merely to suggest here that the inter-associations among the elements of a group have their inhibitory side of the kind shown in our experiments, which causes the factors of a subgroup to hold together and at the same time seem incomplete, which prevents mixture and confusion of groups, and which consequently, in the alternation from one group to another, forces a distinction of the differences from the likenesses up to the limit of this type of ability for the individual. And this analysis of difference as well as likeness seems to be the essence of the reasoning process.

We have found then that in acquiring associations there is involved an inhibitory process which is not the mere result of divided paths, but has some deeper basis as yet unknown. Enough has been said to indicate that this inhibition plays a considerable part in several more complex mental processes. It gives us an internal relationship which seems vital to the higher processes. Further experiments will be reported later, on the analysis of the plan, association by similarity and the reasoning process, and the detailed laws of the inhibition process.

THE PLACE OF STIMULATION IN THE COCHLEA VERSUS FREQUENCY AS A DIRECT DETERMINER OF PITCH

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Our problem is closely related to that of the specific energies of the fibers of the cochlear branch of the auditory nerve. Two types of theory, however, have attributed the pitch of any tone directly to the place of stimulation in the cochlea. The first of these types is represented, for example, by the resonance theory of Helmholtz and by the non-resonance theory of Hurst. Both of these theories associate any given pitch with a somewhat constant section of the cochlea. The second type of the *place* theory, so to designate it, is represented by the non-resonance theory of Emile ter Kuile, which holds that pitch is conditioned by the stimulation of a certain area or extent in the cochlea measured from the outer or fenestra end. The further the impulses extend into the cochlea the greater the extent of stimulation and therefore the lower the pitch of the sensed tone.

Of theories making pitch directly dependent upon frequency of stimulation Max Meyer's non-resonance theory is probably the best example. Some resonance theories also fall under this head in part or entirely; for example, that of Ebbinghaus belongs here.[1] A number of resonance theories are based in part on the resonance idea and in part on that of frequency. Such obvious inconsistencies come to resonance theories devised to support the view of Koenig, that certain tones result from rapid beats of interference.

It seems to me that sufficient experimental and easily verifiable facts are now known to reduce the confusion in this

[1] A theory that should be in a third class by itself is that of Ewald, also a resonance theory, assuming pitch to be determined by the distance of separation of the supposed stationary waves on the membrane in the cochlea.

field and to redefine more specifically our problem. Certain of the 'supplementary' resonance theories, more or less inconsistent with themselves, are no longer necessary to 'explain' any facts known. Experimental evidence has steadily gone against them. In the first place modified resonance theories of the type of Koenig's and Ebbinghaus', devised to account for beats of wide intervals, for 'beat tones,' phase-changing 'middle tones,' other kinds of alleged phase-changing tones, interruption and variation tones have no good reason for their existence. The brilliant experiments conducted by Koenig, Hermann, Dennert, and others, have lost their convincing effect against analysis of tones on the principle of Ohm's law, defended by Helmholtz. More recent experiments, conducted chiefly by K. L. Schaefer and O. Abraham, have shown that the interruption, variation, and the so-called phase-changing tones can be reinforced by physical resonators, and are therefore after all based upon sinus-form vibrations. The origin of these periodicities therefore becomes a physical problem, and has been explained mathematically. The phase-changing 'middle tone' of Hermann is now generally discredited.[1]

The question of the beating of wide intervals is evidently not so easily disposed of, inasmuch as it seems to involve in a measure the matter of tonal fusions and probably the much discussed problem of dissonance. Without trespassing too much upon unsettled problems we are probably safe in saying that the extreme view taken by Koenig on this matter is unnecessary. It is well known now that a beating of difference tones as well as of upper partials makes the generating tones seem to beat. My own experience makes it doubtful to me that the real source of the beating can be singled out with certainty, so that the generators will not themselves seem to beat. Lord Kelvin pointed out that the beats of an imperfectly tuned chord, *e. g.*, 3 : 4 : 5, could sometimes be heard after the primary tones themselves had become inaudible.[2] This beating has been

[1] See my 'Combination Tones and Other Related Auditory Phenomena,' PSYCHOLOGICAL REVIEW SUPPLEMENT, No. 39, p. 73.

[2] *Proc. Royal Soc. of Edin.*, IX., 1878, p. 602.

shown to be due to interfering difference tones.[1] Thus wholly inaudible combination tones may make the generators seem to beat very perceptibly. However we may finally explain dissonance, it does not at all seem necessary now to assume any direct physiological interference of the primary tones of wide, beating intervals. Wundt, who has held to the Ohm's law formulation, has explained certain beats of wide intervals as due to direct stimulation by both primary tones of all the fibers of the auditory nerve. Other writers, as Cross and Goodwin, have attributed them in part to some similar condition produced in the cortex itself. Such suppositions may prove to be useful in some instances and consistent with facts showing that one can experience phase-differences in vibrations falling upon the two ears and that one thereby locates the source of the sound on the side of the advance phase; they do not yet, however, seem to be necessary.

No consistent physiological explanation has yet been given of how the ear could, on the resonance principle, experience *un*analytically the general form of the complex physical wave. Ebbinghaus recognized that such a view contradicts that of the specific energies of the nerve fibers and therefore gave up specific energy and made his well-known modification of the Helmholtzian theory. One may be surprised, however, that scientific men have taken Ebbinghaus seriously at all. To explain how beats may be experienced directly from the form of the objective complex wave Ebbinghaus assumes that the fibers do not have a high degree of elasticity and that they can therefore continue their movements no considerable time after the cessation of the objective impulse.[2] Such fibers he assures us will take up vibrations that are not strictly pendular form (336). On the other hand, and in direct opposition to this, his second assumption, he makes another, 'not without a certain feeling of reserve' (339 ff.)! This third assumption is his well-known doctrine of partial vibration of these same 'inelastic fibers' by means of the formation on them of nodal

[1] Peterson, *op. cit.*, pp. 111 ff.

[2] Grundzüge, 1905, 336–338.

points. Not inferquently does he assume partial vibrations of a high degree. This assumption is to account for the beating of wide intervals and the production of difference tones, when these beats become rapid enough. It also serves him in a rather ingenious explanation of tonal fusions, on the basis of coincident partial vibration of certain fibers. To make plausible this view of partial vibration of so small fibers Ebbinghaus points out that the heavily laden fibers of the contra octave of the piano may give forth 'on the slightest impact their twelfth, yea even their fourteenth, partial tones as a most splendid after-klang of the tone' (340). The contradiction in these assumptions, first of a low degree of elasticity and then of a high degree, is so obvious and so serious as entirely to discredit Ebbinghaus' modification of the Ohm-Helmholtzian theory.

It should be said here that Ebbinghaus' so-called intertone (Zwischenton), which he could not explain on the Helmholtzian theory, does not require for its explanation so extreme a modification of the specific energy resonance hypothesis as he supposed. I do not myself experience anything that can really be called a tone, without greatly extending the meaning of the term. All other experimentalists whom I have consulted on this matter agree with me on this point. I hear rather a beating mass involving more or less clearly the primaries, according to the extent of separation of the latter, and seeming to cover the space, so to speak, between them. If either of the primaries is suddenly stopped the beating complex at once makes a jump to the exact pitch and distinctness of the remaining tone.

Subjective combination tones are now with much plausibility attributed to periodicities arising in the liquids of the inner ear *directly from the primaries* on the principle of superposition of their vibrations, an explanation not dependent—as is that of K. L. Schaefer—on any asymmetrical vibration of the inner membranes though not denying that those membranes *may* play some part in the production of combination tones. *All combination tones are therefore in their origin probably objective to the sensory end organs of hearing.* This

position is strengthened by the establishment of the fact that 'subjective' summation tones actually exist as this explanation requires. In 1907 I proved that such summation tones (some of which were audible) are not difference tones of upper partials as had been maintained by several investigators. This proof rested on counting the beats of summation tones with slightly mistuned auxiliary forks. Krueger and Meyer had already come to the same conclusion on other grounds. On the question of these summation tones, because it was crucial, certain writers had fluctuated not a little.[1]

In connection with vibrations involving phase-changes we find an interesting test of any resonance theory. Early experimenters who supposed that they produced phase-changing vibrations discredited the Helmholtzian view of the resonance hypothesis because they found that they were able to experience such vibrations as tones. Since the reinforcement of such tones by physical resonators has been accomplished this objection has lost its force. In a positive way it has been found[2] that real phase-changing vibrations, when the changes follow closely upon one another, do obliterate the tone. In addition to these facts it has long been known that high tones probably reach their maximum intensity earlier at the beginning of periodic stimulation and certainly decline more rapidly at its close than do low tones. This is all favorable to the view, if it does not actually prove it as Helmholtz urged, that different parts of the ear respond to different periodicities.

Of non-resonance theories that of Hurst assumes specific energies but fails to account for most secondary phenomena of hearing. Emile ter Kuile's theory, also in a measure a place theory, not only fails to explain the origin of most of the subjective combination tones, but is also contradicted by such pathological phenomena as tonal islands. The last-named phenomena, if their existence can be established by careful reports of a few cases thoroughly studied, seem to be fatal to Meyer's theory, the most completely

[1] Peterson, *op. cit.*

[2] *E. g.*, by Exner and Pollak, *Zeitschr. f. Psychol.*, XXXII., 1903, 305–332.

developed probably of all non-resonance theories. The writer has been assured by Dr. G. E. Shambaugh that cases of tonal islands are not infrequent among pathological phenomena of hearing.

It was long ago found, moreover, that destruction of the base of the cochlea, either by pathological conditions or by operation, produces deafness for high tones and that injury to the other end of the cochlea results in deafness for low tones.[1] While these facts, on account of certain difficulties in applying the test, have been questioned, we have nothing to refute them and strong reasons to expect just such results. Recent experiments by Kalischer[2] contradict these facts, but Kalischer's experiments are in many respects unsatisfactory as evidence against specific energies.

Many of the above considerations, as well as others that could be pointed out, seem to require a specific energy theory of hearing. The only reasonably successful specific energy theory thus far put forth is the one based on resonance and strongly advocated by Helmholtz. The Helmholtzian theory as first stated has of course proved inadequate in many respects. The requirements of such a theory now are simpler than formerly, since it has been found that all tones heard—except Ebbinghaus' intertone, if it really can be called a tone—are attributable to periodicities coming to the organ of Corti as objective. Some of these periodicities are secondary, being derived from the primaries, and so occasion the so-called secondary phenomena of hearing. Some such derived periodicities arise externally to the ear and some very probably in the liquids of the cochlea; but they all apparently come about on the principle of superposition of the primary vibrations, above referred to, according to a well-known physical law.[3]

We seem to be at a juncture, then, where physiological theories of hearing have to do, so far as the *existence* of all perceived tones is concerned, principally with the primary phenomena of tonal analysis. If Ewald's view accounts for

[1] See reference in Nagel's *Physiologie des Menschen*, III., 1905, 564–565.

[2] *Arch. f. Physiol.*, 1909, 303–322.

[3] Peterson, *op. cit.*, 103–105.

these phenomena better than does the specific energy hypothesis we should accept it as a better theory. But as yet Ewald has no small degree of difficulty with primary analysis. Meyer's theory obviously has little basis for its existence other than his objection to the assumption of resonators in the ear, if the origin of combination tones suggested above be accepted, and Meyer himself accepts the possibility of such origin.[1] We are in need of some good crucial tests—less doubtful in interpretation than those of Kalischer and of others before him—as to whether tones of different pitch are associated essentially with different and definite sections of cochlear stimulation. A careful post-mortem study of the cochlea of a person who had been deaf to certain tones would be most satisfactory. In the meantime a theory like that of Helmholtz, not taking specific energies too strictly, however, seems to be most helpful.

While it is yet uncertain whether there are structures in the cochlea that can respond sympathetically to tones, the resonance hypothesis has doubtless been strengthened by the demonstration by means of Ewald's *camera acoustica* that a thin rubber membrane .55 mm. by 8.5 mm. suspended in a liquid can be made to respond sympathetically to tones ranging within a compass of six octaves. Ewald of course uses these facts to support his own theory, which, however, does not seem to have any advantages over the specific energy resonance theory, and fails on several points which the latter meets successfully. Ewald's explanation of tonal islands is far from satisfactory; as is also his assumption that pitch is determined by the distance of separation between stationary waves on the membrane. In case of several tones experienced simultaneously so that waves overlap, which distances shall determine the pitch? If a vibration with a strong second partial be given, why should it not be located an octave too high? If the tone is not 'placed' by

[1] He so admitted in his discussion of this paper in the Cleveland meeting. In my paper there I stated that Meyer's theory rests its case on the intensity relations of combination tones and that it has thus far failed to meet the facts on this point as well as the Helmholtzian theory. Professor Meyer, however, informs me that his objection to the assumption of resonators in the ear is really the basis of his theory.

the nearest regular distances between waves, why on hearing a pure tone should one not also hear a tone an octave, or as many octaves as you please, below the given tone? One, as a matter of fact, not only does not but *can* not do this.

We shall now consider three objections to the Helmholtzian theory, objections which have had some weight and some of which we have not yet mentioned in this paper. The first is that based on the intensity relations of combination tones. We shall touch it very briefly. It has been pointed out that combination tones do not frequently have the intensity that we should expect them to have. Hermann pointed out the fact that the tones $c^2 : e^2$ sounding gently give a second difference tone, g^1, which has much greater intensity than the first. He regarded this tone g^1 as belonging to a *second* order, and therefore as being dependent upon the first difference tone. Other critics have proceeded on the same idea. This view, however, is erroneous and inconsistent as I have elsewhere shown.[1] Helmholtz himself at one time assumed various orders of combination tones, in this sense, but Bosanquet pointed out as early as 1881 that this assumption is not necessary.[2] On the principle of superposition of vibrations suggested by Helmholtz in 'Sensations of Tone,' p. 412, all the combination tones spring directly from the primaries, so that the higher orders are not dependent on the lower.[3] The fact that the higher order combination tones may occasionally be more intense than the one of the first order, is not surprising. Nothing could be more natural, since some of these tones occur at a pitch more frequently heard and are therefore mediated by a section of the cochlea more commonly used than that mediating some of the first order combination tones. Again, two or more difference tones frequently coincide and thereby reinforce each other, as Krueger has urged; while some such tones are weakened by others lying below them. Intermediate difference tones are always weak, partly, we should think, because they are obscured by the lower primary tone. When

[1] *Op. cit.*, 23–25.

[2] *Phil. Mag.*, 5th series, XI., 420–436 and 492–506.

[3] It will be noted that Helmholtz suggested several origins of combination tones, some of which are contradictory while others really involve the same principle.

these things are considered the Helmholtzian theory is probably not so unsatisfactory in this regard as has been supposed. While many difficulties of course still remain with respect to this matter of intensity, we should not be unmindful of the difficulties of applying mathematics to matters of this kind. At any rate, no other theory has yet met the intensity difficulties as satisfactorily as the specific energy resonance theory.

The other two points are more simple in their nature, but have given considerable difficulty to the theory under consideration. In 1876 A. M. Mayer reported[1] the fact that tones of considerable intensity, when heard by themselves, may be completely obliterated by lower loud tones; while intense higher tones cannot obliterate lower ones though the latter are very weak. "These results," says Lord Rayleigh, "which are not difficult to verify, involve a serious deduction from the universality of Ohm's law, and must have an important bearing upon other unsettled questions relating to audition."[2]

This objection has for years not been answered satisfactorily by any defender of the specific energy resonance theory. Recently I noticed that the phenomenon is more pronounced for some positions of the sounding forks than for others, when they are held near the ear. In certain relations of the forks the longer vibrations seem to interfere physically with the shorter ones in the outer ear. For occasional positions the higher tone does not disappear at all, though the lower one is very loud. The habit of disregarding upper partials, moreover, seems to make the obliteration more easy and complete in the case of consonant than of dissonant tones. To test whether the obliteration is not due to some sort of physical interference in the middle ear, I applied the stem of the forks to the skull. Under such conditions, it was found, obliteration even of weak tones is very slight or impossible. There can be no doubt that in this case both vibration-series reach either ear in common. If the stem of the higher pitched fork is first applied to the head and then

[1] *Phil. Mag.*, II., 500.

[2] *Theory of Sound*, II., 1896, 445.

that of the lower fork, sounding loudly, when the first has become almost inaudible, one still hears the higher tone very distinctly, and can mark its gradual disappearance. If at this moment the experimenter quickly removes the lower fork, the upper tone does not reappear, showing that its disappearance was really not due to the lower intense tone. The conclusion is inevitable, that the interference of the higher by the lower tone is largely physical, and therefore external to the cochlea. The damping possibly occurs mostly in the ossicles of the middle ear. The phenomenon is consequently not an obstacle to the Helmholtzian theory of tonal analysis.

Our third point relates to the perception of the direction of sounds by means of phase differences, a fact which Lord Rayleigh regards as proof that tonal analysis is central and not peripheral in the sense that Helmholtz and others have assumed. It has unquestionably been established that we locate lateral tones of a period below about 128 vibrations—or *c*—largely on the basis of the phase-differences of the waves impinging upon the two ears, the ear on the near side getting the wave in an advance phase. This phenomenon has been tested under most carefully controlled conditions by Lord Rayleigh and others.[1] By holding, one to each ear, the resonance boxes of two tuning forks which are beating very slowly, so that they alternate in presenting the advance phase of a practically single vibration-series, one finds by a simple method that the sound shifts from side to side, as Lord Rayleigh has reported. The experiment is simple and convincing, though the proof of the perception of phase-differences rests upon tests of a more complex nature. If the tones, in this illustrative experiment, are not too loud, it is impossible with the keenest attention to either box to hear a tone on that side continuously. When the sound has completely left one side, it instantly returns on removal of the fork at the opposite side. When the tones are very loud or of unequal intensity other factors interfere.

The perception of phase-differences does not in the writer's opinion disprove the Helmholtzian resonance hyopthesis.

[1] *Phil. Mag.*, since 1907.

If the elasticity of the resonating structures in the cochlea, whatever they are, is slight, as Helmholtz always maintained, I see no reason why, from some cortical region, note cannot be taken of a slight disparity in phase, so to speak, of the neural impulses from the two ears. Any other theory, even the one favored by Lord Rayleigh—that of Rutherford—if it can be called a theory, must posit an effect in consciousness that is based upon a lack of simultaneity with which the individual impulses from the two ears arrive at some common center.

THE RELATION OF MEMORY ERROR TO TIME INTERVAL[1]

BY K. M. DALLENBACH.

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PROBLEM

It was the 'Aussage' problem, the problem of testimony, and its application to law, that first attracted our attention. In a criminal case a witness is always required to repeat his testimony many times before the final trial. He gives his

[1] The experimental work upon the 'Aussage' method was performed in the psychological laboratory of the University of Illinois in 1909–10, and that upon the 'Geometrical form' method was performed at the University of Pittsburgh in 1910–11. It is with sincerest appreciation that I acknowledge my debt to Prof. J. H. White, of the University of Pittsburgh, and Dr. F. Kuhlmann, formerly of the University of Illinois, for their advice and assistance upon this problem.

testimony before the coroner's jury, the grand jury, the lawyers who coach the witnesses, and finally before the petit jury. In many cases months pass before the witness gives his final testimony. In the 'Aussage' problem the relation between capacity and reliability of the report to the time interval has been thoroughly worked out. In reviewing this field the question arose in our minds: what becomes of the errors in memory due to the lapse of time? The relation of memory to the time interval has been fairly definitely established, but, so far as we have been able to discover, no work has been done upon the relation of memory error to the time interval. It is common knowledge that no two people will see and report the same event in the same terms, nor will they agree upon all points in their narration. There is a personal equation, an individual difference, which gives rise immediately to a disagreement of testimony. Both observers cannot be right, both will be in error upon some point. It is with this phenomenon that we are concerned. We are endeavoring to discover the relation of the error in memory to the time interval.

Aussage Method

Observers

In taking up this problem an effort was made to simplify it as far as possible. Our observers were therefore limited to college men ranging between the ages of twenty and thirty years. This precaution was taken because differences in age, sex, training, and intelligence, have been found by many experimenters in memory to give different results (2, 10, 20, 26, 28, 29).

Materials

Many types of material have been suggested for the experimenter in this field: the object test, the rumor test, the event test, the picture test, the moving picture test, and the dramatic test. Of all these methods, the picture test was found to be the simplest and the most adequate for our present purposes and equipment. The form of picture test that had been developed by Stern (26, 27), Borst (5, 6), and others, was adopted. The pictures (see Plate I. and Plate II.)

were chosen because of their simplicity, and their naturalness and vividness of outline. For the sake of brevity the picture shown in Plate I. shall be called the 'adult' picture, and that in Plate II. the 'juvenile' picture.

Fifteen observers worked and reported upon the 'adult' picture and as a check upon the results obtained from these observers five others, following exactly the same methods, worked and reported upon the 'juvenile' picture.

Exposure Time

The pictures were exposed for one minute. This interval, indeed, was too short for some of the observers, and too long for others, but for the average of our observers it was found to be of sufficient length to permit of at least one examination of every detail in the picture. To insure greater facility in memorizing the observer was warned at the end of half a minute by the words, "your time is half over." This division of the exposure time was a great aid to the observer.

Intervals between Recalls

The time intervals, the lengths of the periods between reports, were more or less arbitrarily decided. An immediate report, a five-day report, a fifteen-day report, and a forty-five-day report were taken. These intervals were chosen mainly because, after the five-day report, they increased by a multiple of three. The effect of the length of the time interval has not been definitely established as yet, and most experimenters in this field have likewise been more or less dogmatic, and have adopted an interval most suitable to their purposes and conveniences without any scientific justification for their choice.

Kinds of Recalls

The report that the observers were required to give was of two kinds, the narration, and the deposition.

The narration was a free account given by the observer in writing without any external suggestion or comment upon the part of the experimenter. The observer could follow whatever bent he chose, his only instructions being to give

a complete description of the picture. There was no time limit set for this; the observer was given all the time that he required. At every report the narration was given first.

The deposition corresponding to the 'Verhör' of Stern, 'Prüfung' of Wreschner, 'Interrogatoire' of Miss Borst, and the 'Questionnaire' of Binet was next taken. It consisted of a series of sixty stereotyped questions, which were carefully prepared and which we thought completely covered all of the features in the picture. The questions were presented orally, and the answers were given in writing. The observers were instructed to give as short and as concise answers as possible and to designate the degrees of certainty of their answers by the symbols *a*, *c*, and *e*; referring respectively to answers that were just a little better than a mere guess, to answers of which the observer was fairly certain, and to answers of which he was absolutely certain. Each observer was further instructed, if a case arose in which he did not know the answer, and knew that he did not know it, to return such a reply, and never under any circumstances to guess. Also, to use the symbol *e* only when he would be willing to give his sworn oath. It was further made clear to the observer that having once given an answer it would be impossible to change it until a subsequent report. This last mentioned precaution was found to be necessary, for many of the questions were suggestive in their nature, and would give clews to the answers of some of the previous questions. The questions were so arranged that any suggestion would normally refer backwards, *i. e.*, to an answer of some preceding question.

Conduct of Experiment

The observers were taken one at a time and informed of the nature of the problem, the method of procedure, the form of material, the length of exposure, the nature of the report, the periods of recall, and the character and nature of the symbols used in making the report. The picture was then placed in the observer's hands, and he was allowed perfect freedom in making his observation. As stated before, he was, however, warned when half of the exposure time had elapsed. After the exposure the narration and deposition were taken.

Method of Grading Data

The computation of the data thus obtained was considered separately. In grading the data given in narration, individual facts as stated by the observer were taken as the unit of recall. For convenience, these units were enclosed in parentheses and marked plus or minus according as they were right or wrong. After the units were thus separated, the data were worked over several times for the purpose of eliminating the repetitions and the inferences. By an inference is meant everything that is of the nature of a personal interpretation. It was thought best to eliminate all such units, since individuals would vary greatly in these interpretations, and since it would be difficult to decide in any given case whether to call them right or wrong. In figuring the per cent. of error each observer and each recall were considered separately. The relation was then computed on the basis of the total amount recalled, the plus and minus units taken together, and the number of errors made in that recall. For example Observer No. 1 on the 'adult' picture gave in his immediate recall one hundred and fourteen elements. Fourteen of these were eliminated because they were either repetitions or inferences, and from the remaining one hundred thirteen were marked as incorrect. The per cent. of error in this recall figuring upon this basis was then thirteen. This procedure was followed throughout the grading of the data. The per cent. of error was computed upon a basis of the number of elements, minus the repetitions and inferences, reported in each recall.

The deposition is more artificial to be sure than the narration just mentioned, but it is the method that one meets in the court room, in the cross examination before the coroner's jury, the grand jury, and at the trial. It more nearly represents the conditions, then, under which legal testimony is given. As in the previous case each observer and each recall were considered separately. There are sixty questions on each picture, each question being counted as an element, and the answers were graded according as they were right or wrong. Some questions, such as 'How many saucers are there on the table?' there being three, involve in their answer

more than one object. As was often the case only two were remembered and the answer 'two' returned: in such cases the answer was two thirds correct, and was so graded. When the answer 'I do not know' was given to a question it was considered neither as an error nor as correct. The question was in such cases omitted from consideration. The per cent. of error was computed upon the basis of the number of errors made and the number of positive answers returned.

Results

The results from the narration describing the separate pictures are tabulated in the table given below.

TABLE I

Recall	'Adult' Picture		'Juvenile' Picture	
	Total Number of Elements in Recall	Per Cent. Error[1]	Total Number of Elements in Recall	Per Cent. Error[1]
Immediate....	765	10.5	263	8.0
5 days.....	735	14.3	245	12.2
15 days.....	750	18.0	217	16.1
45 days.....	569	22.4	204	20.0

[1] Not only do the composite results show this relation of the memory error to the time interval, but so also do the results of the individual observers.

The results from the deposition for the two pictures are given in the two tables below.

TABLE II

RESULTS OF FIFTEEN OBSERVERS ON THE 'ADULT' PICTURE

Recall	Total Number Questions Answered	Per Cent. Error	*a*		*c*		*e*	
			Total No. Answers	Per Cent. Error	Total No. Answers	Per Cent. Error	Total No. Answers	Per Cent. Error
Immediate.....	880	14.1	44	48.2	212	28.3	628	6.7
5 days.....	855	18.2	41	66.6	229	30.7	585	10.0
15 days.....	854	20.7	46	64.5	243	25.3	565	15.4
45 days.....	801	22.4	30	55.0	240	31.4	531	19.5
				Av. % w.[1]		Av. % w.[1]		Av. % w.[1]
Totals.....	3,390		161	58.6	924	28.9	2,305	12.9

[1] Average per cent. of error.

TABLE III

RESULTS OF FIVE OBSERVERS ON THE 'JUVENILE' PICTURE

Recall	Total Number Questions Answered	Per Cent. Error	*a*		*c*		*e*	
			Total No. Answers	Per Cent. Error	Total No. Answers	Per Cent. Error	Total No. Answers	Per Cent. Error
Immediate....	289	11.4	13	46.1	86	23.2	190	3.6
5 days.....	267	15.7	14	35.7	82	24.3	171	9.3
15 days.....	260	16.9	14	50.0	86	23.2	160	10.6
45 days.....	249	18.1	18	55.5	87	16.0	144	11.1
Totals.....	1,065		59	Av. % w.[1] 46.8	341	Av. % w.[1] 21.7	665	Av. % w.[1] 8.6

[1] Average per cent. of error.

The results of the narration (Table I.) show that the per cent. of error increases with the time interval, and that the increase is regular, being about four per cent. at each recall.

The results of the deposition (Table II. and Table III.) likewise show an increase of error with the time interval. The increase is not regular as in the case of the narrations, but rises rapidly at the second recall. The increase of error is as great in the first five days as it is in the next forty.

In Tables II. and III. the answers are also divided according to their relative degree of certainty. The number of such answers are shown in the tables together with the per cent. of error made in those answers. Of the 3,390 replies given by the observers upon the 'adult' picture (Table II.) 161 or 4.8 per cent. are *a* replies, 58.6 per cent. of which are wrong; 924 or 27.3 per cent. are *c* replies, 28.9 per cent. of which are wrong; and 2,305 or 67.9 per cent. are *e* replies, 12.9 per cent. of which are wrong. Of the 1,065 replies upon the 'juvenile' picture (Table III.) 59 or 5.5 per cent. are *a* replies, 46.8 per cent. of which are wrong; 341 or 32.1 per cent. are *c* replies, 21.7 per cent. of which are wrong; and 665 or 62.4 per cent. are *e* replies, 8.6 per cent. of which are wrong.

In both the narration and deposition the error was smaller upon the 'juvenile' picture than upon the 'adult' picture, though the increase of error with the time interval was proportional in both cases. This probably can be explained by

the fact that the 'adult' picture (Plate I.) contains many things that are unfamiliar, whereas the 'juvenile' picture (Plate II.) fits into the background of our daily experiences.

One salient criticism can be brought against the experiment. It is that too much depends upon the experimenter's interpretation of the data. In many cases the narration and answers to the questions were so worded that it was with the greatest difficulty that a decision was made as to whether the answer was right or wrong. In our effort to eliminate this difficulty we sought for a test that should be sufficiently simple and concrete to permit of exact grading, grading approaching mathematical precision. We therefore adopted a method of using simple geometrical figures, which method we have chosen to call the 'geometrical form' method.

Geometrical Form Method

Observers

As in the 'Aussage' method, and for like reasons, we limited our observers to men with a college training, ranging between the ages twenty to thirty years. Nineteen observers were used in this method.

Materials

The materials for this method consisted of geometrical figures drawn upon a number of white exposure cards. Only one figure was drawn upon each exposure card. A series consisted of twelve of these cards arranged in serial order with twelve blank cards so that when the exposure was made a blank card and a figure card were alternately seen. There were in each series three geometrical figures which differed from one another in position, size, and color. The position of the figures varied from the horizontal to angles of forty-five, ninety, one hundred thirty-five, one hundred eighty, two hundred twenty-five, two hundred seventy, and three hundred fifteen degrees. But two sizes were used in each series, a large figure and a small one. The large figures were three times greater in area than the small. Each figure was colored, the seven standard spectral colors were used for this. We

PLATE I

PLATE II

were careful to obtain well saturated colors so that there should be no difficulty on the part of the observers in recognizing them.

Exposure Time

Each card, blank and figure card alike, was exposed for three seconds. This exposure was repeated three times in immediate succession.

Intervals between Recalls

The type of material used in these tests was very poor in arousing associations, and the figures passed very rapidly from the mind. Therefore the reports did not extend over a long time interval. They were given at the following intervals: immediately, at the end of one day, three days, and six days.

Recall

A report consisted in the observer's drawing and coloring all the geometrical figures of a given series that he could remember, and also designating the degree of certainty of his replies by the symbols *a*, *c*, and *e*, the significance of which we have before described.

Conduct of Experiment

As in the 'Aussage' method the observers were taken one at a time and informed of the nature of the problem, the method of procedure, the materials, the length of exposure, the nature of the report, the periods of report, and the nature and significance of the symbols to be used in making the reports. A series was exposed to but one observer at a time. The material was presented by hand, and the exposure time was measured by a metronome.

Method of Grading Data

In each series there were four elements involved in the recall of each figure, namely: shape, position, size, and color. There were then forty-eight elements in each recall. Every report was graded according as its elements were correct or not, and the per cent. of error was figured on a basis of the number of elements reported, and the number of errors made.

Results

The results of the first six series under this method are tabulated in Table IV. given below.

TABLE IV

Recall	Total Number of Elements Reported	Per Cent. Error	*a*		*c*		*e*	
			Total Number	Per Cent. Error	Total Number	Per Cent. Error	Total Number	Per Cent. Error
Immediate.....	2,822	16.8	117	59.8	342	38.5	2,363	11.6
One day.....	2,629	21.3	110	53.6	360	46.1	2,159	15.5
Three days.....	2,547	23.0	104	49.0	341	53.3	2,102	12.2
Six days.....	2,531	25.0	102	65.7	313	56.2	2,116	13.6
Totals.....	10,529		433	57.0[1]	1,356	48.8[1]	8,740	13.2[1]

[1] Average per cent. of error.

In computing the data thus gathered we discovered that we could not,—as we had hoped,—grade the data with mathematical precision. The question often arose: to which figure in the original material did a specific figure in the observer's report refer? The relation was not always clear, and could be more or less arbitrarily decided. In order to systematize the grading, and to eliminate this factor as nearly as possible we adopted the following rule, viz.: in every case so to grade the data that the per cent. of error would be minimal. The benefit of the doubt was in every case given to the observer.

In order to eliminate entirely this factor, four other series were given the observers, the geometrical figures of which differed throughout. With the adoption of differently shaped figures it seemed advisable to eliminate the element of size. Size is only a matter of comparison, and in these series no comparison was at hand, since no two figures were alike. Therefore only three elements, position, shape, and color, were here considered. These series differed from the preceding six not only in the fact that the element of size was omitted, but also in the number of figure cards in each series, and in the number of times each series was presented to the observer. In Series VII. and IX. there were eleven figure cards, while in series VIII. and X. there were only ten. Series VII.,

which was composed of geometrical figures that had been previously employed in the first six series, was presented to the observer five times; whereas Series VIII., IX., and X., which were composed entirely of unfamiliar figures, were exposed ten, six, and four times respectively.

The same observers served upon these series as upon the preceding six. They were informed that the figures would differ throughout the series, and that the number of presentations would also vary, a warning being given on the next to last and last presentation by the words, 'next to last exposure,' and 'last exposure.'

The results of these series are tabulated in Table V. given below.

TABLE V

Recall	Total Number of Elements Reported	Per Cent. Error	*a*		*c*		*e*	
			Total Number	Per Cent. Error	Total Number	Per Cent. Error	Total Number	Per Cent. Error
Immediate.....	654	7.1	3	33.3	12	58.3	639	6.1
One day.....	638	9.8	0	00.0	12	50.0	626	9.1
Three days.....	632	11.8	1	100.0	16	31.2	615	11.1
Six days.....	612	13.7	2	50.5	16	56.2	594	12.5
Totals.....	2,536		6	61.1[1]	56	48.9[1]	2,474	9.7[1]

[1] Average per cent. of error.

The results of the 'geometrical form' method show that the per cent. of error increases with the time interval; and that this increase is, as shown by Table IV. and also, though in a less degree, by Table V., very great at the second recall. At the third and last recall the increase is more gradual. The increase of error in the first twenty-four hours is greater than that of the next five days.

In both tables, IV. and V., the answers are divided according to their degree of surety. The results of Table IV. closely agree with those of the deposition under the 'Aussage' method. Of the 10,529 elements reported upon in these series, 433 or 4.1 per cent. were marked *a*, 57.0 per cent. of which were wrong; 1,356 or 12.9 per cent. were marked *c*, 48.8 per cent. of which were wrong; and 8,740 or 83.0 per cent. were marked *e*, 13.2 per cent. of which were wrong.

The results shown in Table V. differ greatly from these. Here, of the 2,536 elements reported, only 6 or 0.2 per cent. were marked *a*, 61.1 per cent. of which were wrong; 56 or 2.2 per cent. were marked *c*, 48.9 per cent. of which were wrong; and 2,474 or 97.6 per cent. were marked *e*, 9.7 per cent. of which were wrong. This discrepancy can be accounted for by the fact that the series composing this table were exposed so many times to the observer that he was able completely to grasp the material. This is not only shown by the number of *e* replies, but also in the correspondingly low percentage of error made in the initial and in each of the subsequent reports.

The errors were further classified according to color, position, size, and shape. The results of the last four series were omitted because they were unlike the first six in respect to the number of geometrical figures upon the exposure cards, to the number of times the series was presented to the observer, and to the number of elements upon each card. To keep the data homogeneous only the first six series were used in the computation of the data which appears in the table below.

Table VI

The Number and Relative Per Cent. of Errors Among the Elements: Color, Position, Size, and Shape

Recall	Total Number of Errors	Relative Per Cent. of Error Made in			
		Color	Position	Size	Shape
Immediate.....	475	44.0	36.8	12.6	6.6
One day.....	560	42.7	37.5	13.0	6.8
Three days.....	588	44.0	34.7	13.9	7.4
Six days.....	632	44.5	33.6	14.4	7.5
Totals.....	2,255	Average 43.8	35.6	13.5	7.1

The results of this table show that the greatest number of errors were made in color. Of all the errors made in these series about half were in color, over a third were in position, an eighth in size, and nearly a twelfth in shape.

The errors in color were further analyzed and classified according to hue. These data are shown below in Table VII.

TABLE VII

Recall	Total Number of Errors Made in Color	Blue-Green	Relative Per Cent. of Errors Made in					
			Orange	Light-Green	Blue	Violet	Red	Yellow
Immediate..	209	30.6	16.1	16.2	12.4	9.0	7.1	8.1
One day....	239	25.9	19.6	16.7	11.2	11.2	7.5	6.6
Three days.	259	23.8	19.3	15.8	14.7	11.6	8.5	7.3
Six days...	281	22.4	16.7	14.6	17.0	11.7	9.9	7.3
Totals.....	988	Average 25.7	17.9	15.8	13.8	10.9	8.3	7.3

The results of Table VII. show that the per cent. of error in color decreases from blue-green, in which over a quarter of the errors were made, through orange, light green, blue, violet, red, to yellow, in which the least number of errors were made.

CONCLUSIONS

The experiment agrees in its several parts, thereby leading us to the following conclusions, namely:

1. That there is a relation between memory error and the time interval.

2. That the memory error increases with the time interval.

3. That the errors in memory increase very rapidly at first and then more gradually as the time interval becomes greater.

4. That in this respect the curve of the errors in memory conversely resembles the Ebbinghaus memory curve.

5. That practically applied our results establish the fact that a witness gives a more faithful report the nearer his report or testimony is given to the day of the experience.

6. That the degree of certainty of the observer's replies bears a direct relation to the fidelity of the answer; and that this relation remains constant with the time interval.

7. That the greatest number of errors are made in color, the next in position, the next in size, and the last in shape; and that this relation of error between the four elements remains constant to the time interval.

8. That the greatest number of errors in color are made in the green shades, and the least number of errors are made in yellow; and that this relation between the errors in color remains constant to the interval.

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